REMARKS

ON

SOME OF THE OPINIONS

OF

Dr. RUSH

RESPECTING

THE YELLOW FEVER

WHICH PREVAILED IN THE

CITY OF PHILADELPHIA

In the Year 1793.

By WILLLIAM PATTERSON, M.D.

MEMBER OF THE ROYAL IRISH ACADEMY, CORREPSONDING MEMBER OF THE LONDON MEDICAL SOCIETY, AND SURGEON TO THE CITY AND COUNTY OF LONDONDERRY INFIRMARY.

" Dubitationes, in codicillos relatæ, totidem ſpongiæ ſunt, quæ incre-
" menta ſcientiæ perpetua ad ſe ſugant, et alliciant.——*Bacon de Dig. at Aug. Scient.* Lib. iii. Cap. iv.

LONDON-DERRY:

PRINTED BY G DOUGLAS.

1795.

TO

The Preſident

AND

The other Members

OF

The College of Phyſicians of Philadelphia,

AS

Competent and liberal Judges,

THESE REMARKS ARE SUBMITTED:

AND

To them alſo,

AS

A Teſtimony of Reſpect and Eſteem,

THE ESSAY IS INSCRIBED,

By their humble Servant,

Wm. Patterſon.

LONDONDERRY,
Aug. 31, 1795.

PREFACE.

AT this period of general information, and of widely circulating intelligence, it would be ſuperfluous to offer any arguments to ſolicit the Reader's attention towards a caſe, in which the miſery and happineſs of ſocial inſtitutions are placed in a balance. In this woful ſtate of ſuſpence is a community, when aſſailed by an impetuous *Epidemick*—a ſtate to which the human mind cannot advert, even in a country remote from the ſcene, without ſerious and condoling reflections.

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Impreſſed with this ſentiment, I muſt conſequently regret, that ſo great a contrariety of opinion ſubſiſted amongſt thoſe who were the guardians of health in Philadelphia; and, therefore, I am imboldened to ſtep forward to enquire into the grounds on which each party erected its claims of validity. Not that I here preſume to caſt a deciſive influence into either ſide of the queſtion; but, that I entertain a hope, by promoting diſcuſſion, to be an additional means of clearing an important ſubject from the ambiguity in which different views have involved it.

Various reaſons point out Dr. Ruſh's book as the moſt proper for our purpoſe. It is very full on the ſubject; ſtates the different opinions; and inſiſts on a certain mode of practice, on the footing of its being *new* and *almoſt infallible*—a mode which to me appears not only highly adventurous in the yellow fever, but, if generally adopted and applied to other varieties of fever, likely to prove deſtructive.

Before entering, however, upon the intended examination, ſome preliminary obſervations ſeem neceſſary; and in this view, the perplexed ſtate

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of mind, under which Dr. Ruſh generally laboured, attracts our firſt notice. "I lay down," ſays the Doctor, "in conformity to habit only, for my bed ceaſed to afford me reſt or refreſhment. When it was evening, I wiſhed for morning; and when it was morning, the proſpect of the labours of the day cauſed me to wiſh for the return of evening." So great was his diſcompoſure, that he confeſſes he forgot the uſual laws which regulate the intercourſe of man with man in the ſtreets, and upon the publick roads in the neighbourhood of the city.

The multitude of profeſſional calls, likewiſe, unhinged him not a little. So preſſing have they been at times, that he has been obliged, in ſpite of his humanity, to extricate himſelf from ſtoppages in the ſtreet, by driving his chair haſtily out of the reach of the importunate. Together with the numerous patients which he viſited abroad, he frequently preſcribed for 150 perſons daily, in his own houſe, during five or ſix weeks; and had but little time at night, on account of anſwering letters, to copy into his note-book the haſty obſervations made in the day, which he had marked with a pencil in a pocket-book in ſick rooms, or in his carriage.

Practice, under ſuch circumſtances, is well repreſented by the learned Dr. Zimmerman; a ſummary of whoſe ſentiments I ſhall here take the liberty of introducing: Continual fatigue, and a great number of patients, are conſiderable obſtacles to prevent a phyſician, who is in great vogue, from making obſervations as he ought to do, by reflecting deliberately on each. The phyſician who viſits the moſt patients, and he, who, in the ſame town, viſits the feweſt, may very often be ſaid to *ſee* the ſame number. He, who is much employed, ſees theſe patients in a ſuperficial manner; whilſt he, who has leſs to do, conſiders each caſe at his leiſure, and with more attention.

A phyſician, who is conſtantly buſied, *ſees* too much, and does not *think* enough. The rapidity, with which objects ſtrike him, does not ſuffer him to fix on any one ſufficiently; and, therefore, his mind retains only a confuſed impreſſion, or an obſcure remembrance of all. Such a practitioner cannot enter into any of the minute circumſtances of a caſe, and conſequently cannot adapt his preſcriptions to its different ſtages. It has been aptly ſaid, that the phyſician,

an, who is employed night and day amongſt the ſick, like the prieſt who is inceſſantly running about with the ſacrament, ſees indeed a great many *patients*, but not a ſingle *diſeaſe*; which muſt be the caſe, whether his patients all labour under the ſame diſeaſe, or that each is affected with a different one—And it is the opinion of another celebrated author, John Brown, (to whoſe medical creed Dr. Ruſh ſeems a ſubſcriber) that fevers require more viſits from the phyſician than are commonly either beſtowed or demanded, and often a good deal of watching. Nay, Dr. R. himſelf owns, that the number of patients, who crowded in upon him, particularly in the firſt week of September, prevented him attending as much to each of them, as was neceſſary to determine upon the propriety of his principal expedient in the cure.

In addition to the effects of thoſe ſtates of perturbation, I am ſorry to obſerve, that the weight of Dr. Ruſh's opinions ſuffers farther diminution, by his departing ſo much from the principles of liberality, as to refuſe conſulting with his medical brethren. That a conference between two phyſicians differing in theory and

practice, forgetful at the ſame time of juſtice and humanity, can neither be uſeful nor honourable, I admit; but, that the gentlemen of the faculty in Philadelphia, who differed from Dr. Ruſh, were deſtitute of theſe moral feelings, I cannot for a moment ſuppoſe. I am, therefore, led to believe, that they would not involve their patients in the conſequences of a private quarrel, with which thoſe patients have not properly any concern.

How pleaſing would it have been to have had reaſon to ſuppoſe, that Dr. Ruſh was governed with reſpect to his brethren, by the following ſentiments of the late amiable Dr. Gregory of Edinburgh! "Many advantages," ſays this admired writer, "ariſe from phyſicians conſulting together, who are men of candour, and mutually confident of each others honour. A remedy may occur to one which did not to another; and a phyſician may want reſolution, or ſufficient confidence in his own opinion to preſcribe a powerful but dangerous remedy, on which, however, the life of his patient may depend; in this caſe, the concurrent opinion of his brethren may determine his conduct."

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Nothing but the want of mutual confidence ſeems a ground to juſtify any phyſician for refuſing to conſult with another, when he is required to do ſo. If he is conſcious he cannot behave with temper, and that his paſſions are ſo rankled as to impair his judgment, he may and ought to refuſe it — "It is a phyſician's duty to do every thing in his power, that is not morally criminal, to ſave the life of his patient, and to ſearch for remedies from every ſource, and from every hand, however mean, and in many reſpects contemptible." It is doubly his duty to act thus in a raging diſtemper, which breaks into not only the *pauperum tabernas*, but the *regum turres*.

The firſt object of our reſearch is, the *Fons et Origo Mali*; and it is a principal one; for until the germinating cauſe of malignant diſeaſes be aſcertained, neither the means of prevention, nor the methods of cure, can be rationally tried, or prudentially adminiſtered. It is of the firſt conſequence to determine whether a peſtilential fever be imported, or engendered in the country in which it appears. In the former caſe, the various quarantine rules will be neceſſary; whilſt

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in the latter, thoſe rules would be not only ſuperfluous but hurtful; and the means of prevention, or of remedy, would depend entirely on interiour regulations.

An inquiry into this point is intereſting even to the inhabitants of Great Britain and Ireland; although Dr. Ruſh entertains an idea, that neither of theſe iſlands has ever been infected with the yellow fever. But, from indubitable authority, it is certain, that this fever has been imported from America into England, as ſtated in the following Obſervations on Contagion.

With reſpect to the importation of the diſorder into Ireland, I cannot furniſh any inſtance. But, ſince it has been introduced into England, whoſe climate does not materially differ from that of Ireland, I ſee no good reaſon why the ſame channels might not conduct it into the one country, as well as into the other; and Dr. Ruſh himſelf conſiders both countries warm enough to favour the propagation of an imported contagion. The cauſe, perhaps, which has hitherto exempted Ireland (as far as I know) from the introduction of the malady, may, in a great

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meaſure, be owing to her harbours being leſs frequented by the great ſources of contagion, ſhips of war, and crouded veſſels returning from the places of its origin and prevalency; which, like electrical conductors, ſerve to convey the peſtiferous *aura* into its proper receptacle.

The country which admits the introduction of the true peſtilence, cannot ſurely be reckoned ſecure from the incurſions of a peſtilential fever; and that Ireland has had her viſitations from the plague, is but too certain. Beſide, if, as Dr. Ruſh has elſewhere obſerved, the climate of Pennſylvania be a compound of all the climates in the world; and, if a diſeaſe be admiſſible into ſuch a climate, may not Ireland, whoſe weather conſiſts of conſiderable variety, be obnoxious to the ſame diſeaſe?

As, however, there is reaſon to ſuſpect that fever is a diſeaſe radically and eſſentially the ſame in all countries; and, as the proximate cauſe muſt conſequently be in every caſe the ſame likewiſe (which Dr. Ruſh maintains); I conclude, that it behoves every phyſician to admit with caution doctrines which militate againſt this

this principle—whence ariſes one of the ſtrongeſt motives for entering upon the intended examination.

The following ABSTRACT is publiſhed to exemplify the extent of Alarm, from a certainty of the diſtreſſes in Philadelphia; and to ſhow the pains which were taken, in Londonderry, to regulate the impreſſions made by that alarm, as far as our influence could reach, ſo as to prevent a blind temerity on the one hand, and a groundleſs fear on the other; the effects of which, in caſes of danger, are inevitably prejudicial.

But the alarm was not confined to this place; the city of London felt it likewiſe. For a fever having broke out, in autumn 1793, in that diviſion of Newgate, called the ſtate ſide of the priſon, and of which Lord George Gordon, &c. died, a publick alarm aroſe, which was greatly augmented by the recent accounts of the fatality of the yellow fever in the Weſt Indies, and North American continent. So great was the apprehenſion on this occaſion, that a ſpecial order was given for a phyſician to viſit the whole of Newgate, and to make a report of the health of the priſoners.

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The ſtriking traits of reſemblance which Dr. Ruſh has pointed out as ſubſiſting between the plague, ſmall-pox, and yellow fever, impreſs us with a ſtrong idea of the fomes of the latter diſorder being durable and powerful. Nay, he concludes, and with high ſatisfaction, that he has eſtabliſhed the analogy between the variolous contagion, and that of the fever—The ſimilitude alſo, which he notes between the yellow fever and typhus, will give conſiderable force to ſome of the following remarks.

In ſhort—Notwithſtanding the numerous examples of the direful effects of contagion, and notwithſtanding the many efforts of ingenious men to diſcover its nature and origin, the ſubject yet remains in ſo mortifying an obſcurity, that every attempt to draw aſide the veil, has a claim on the candour of thoſe readers, who feel an intereſt in whatever profeſſes to mitigate human miſery, and to increaſe the utility of a liberal profeſſion.

ABSTRACT

OF THE

PROCEEDINGS at LONDON-DERRY

IN

CONSEQUENCE OF THE PREVALENCE OF A MALIGNANT FEVER IN PHILADELPHIA IN THE YEAR 1793.

AT a meeting of the Citizens of *London-Derry*, held in the Exchange of ſaid City on Friday 14th November 1793, it was reſolved,

That, conſidering the frequent intercourſe between London-Derry and America, it is peculiarly incumbent on the inhabitants of this City, and the neighbouring Country, to uſe every precaution leſt the dreadful diſtemper now raging in Philadelphia ſhould be communicated to this kingdom.

And, as it appears probable, that notwithſtanding the wiſe orders which have been iſſued by Government, and the ſtricteſt attention to

thoſe orders on the part of the reſpective Officers, a contagion may ſtill be communicated by the landing of perſons, or goods, on thoſe parts of the coaſt where no ſuch Officers are ſtationed, the Mayor is hereby requeſted to apply to Government, in the name of the Citizens of Derry, praying, That two or more armed Cutters may be ſent as ſpeedily as poſſible to cruize on the coaſt, for the purpoſe of preventing ſuch landing, and enforcing the regulations of quarantine on thoſe veſſels which may about this time be daily expected.

Alſo; as it may be neceſſary that the people who live near the coaſt may be apprized of the hazards they will incur, and the deplorable evils they may bring upon their families and their country, by going on board of, or having any communication whatever with, ſuch veſſels, it is judged expedient, that a Committee be appointed for the purpoſe of drawing up and diſſeminating ſuch inſtructions, as may put all perſons on their guard, and direct their conduct at this alarming juncture.

A Committee, conſiſting of nine Citizens, being accordingly appointed, the preparation of a Report devolved principally on three of them, who were in a great meaſure influenced by the following

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CONSIDERATIONS.

WHEN we reflect on the calamities attending the propogation of a peſtilential diſeaſe in a nation, the ſmalleſt probability of its introduction muſt be regarded with anxiety, and conſequently ſhould be oppoſed with firmneſs. The dread of contracting a diſtemper generally mortal, and of periſhing deprived of the uſual conſolations in other maladies, predominates ſo irreſiſtibly, as to diſſolve the ſocial ties of humanity; and often, even in the moſt poliſhed cities of Europe, occaſions ſcenes of horror and anarchy, unparalleled in countries reckoned leſs civilized.

To eradicate this terror by declaring (as has been done even with reſpect to the plague) that a diſeaſe exerting the moſt malignant effects is not contagious, and is eaſily cured, muſt be a vain, and, in its conſequences, a deſtructive expedient. The common ſenſe of mankind rejects propoſitions ſo inconſiſtent with hiſtorical records; or if, in the beginning of a peſtilential diſtemper, the miſguided multitude ſhould become dupes to an ill-founded doctrine that flatters their wiſhes, the illuſion, as the ſeaſon of evil advances, will be diſſipated by the woful conviction of hourly experience.

But although the dread of contagion neither can, nor ought to be extinguiſhed in the mind of man; it certainly may be moderated, and directed to uſeful purpoſes. Reduced within proper bounds, it may be equally ſubſervient to the ſecuring ſubmiſſion to regulations calculated for the general ſafety, and to the encouragement of thoſe, who are obliged to expoſe themſelves in the publick ſervice.

The approach of a peſtilential diſtemper, or the poſſibility of it, inſtead of plunging us into deſpondence, ſhould arouſe us to vigilance and precaution. Timely meaſures may prevent its acceſs; or cannot fail to mitigate its attendant evils, and reſtrain its devaſtation. It cannot become epidemical without the concurrence of a certain ſtate of the air, which may happen to be remote. Several perſons eſcape infection in ſeaſons the moſt favourable to its propagation; and ſo far is even the actual peſtilence from being infallibly mortal, that, upon the whole, a conſiderable proportion of the infected recover.

Theſe are conſolatory facts, drawn from experienced obſervers; and they are facts which may be fitly oppoſed to the popular notions of contagion, ſo apt to affect the imagination in moments of conſternation and diſmay. They may contribute to animate thoſe, who, by their ſta-

ſtations, are called upon to face danger for the publick welfare; but they ſhould never be ſo miſapplied, as to encourage a wanton temerity, or to countenance the neglect of human precautions.

In the ſtate of information, in which we at preſent ſtand, reſpecting the ſubject under conſideration, it appears that the conduct of the magiſtrate ought to be regulated on the ſuppoſition, that the condition of the air, if not in perfect concurrence with the apprehended contagion, might not be in a temperature to obſtruct its ingreſs; and the private individual ought not too confidently to reckon himſelf conſtitutionally exempt from infection. Both may, indeed, happen to be miſtaken, and the precautions of both may poſſibly turn out ſuperfluous; but where publick and private ſafety is ſo intimately concerned, preſervative means are juſtifiable, though grounded on mere probability; for if deferred until doubt give place to conviction, they will come too late.

For theſe, and other cogent reaſons, we conclude that it is abſolutely neceſſary to co-operate with the civil power, in early and ſuitable proviſions againſt the introduction of ſo deſtructive a malady. But, as the act of quarantine is deſigned againſt the true plague, and as the Philadelphia

delphia fever, however malignant, may be conſidered in a different point of view; it appears requiſite to relax, in ſome degree, the rigours of performance; eſpecially ſince it is well known, that ſevere reſtraints in this caſe have been productive of dangerous infringements and breaches. On theſe occaſions, it ought to be a leading principle, as far as may be conſiſtent with the publick ſafety, to avoid multiplying the limitations and embarraſſments on commerce. In times of general apprehenſion, merchants are unavoidably ſubject to many delays, vexations, and loſſes; it were cruel, then, as well as impolitick, to ſuperadd reſtrictions, either unneceſſary, or inadequate to the intended purpoſe.

When the whole crew and paſſengers (the latter of whom are but few *from* America to Ireland) arrive in good health, very little danger need be apprehended from their perſons, or even from the apparel which had been in daily uſe, on the voyage, as they may be ſuppoſed ſufficiently purified by ventilation. But the caſe is different with reſpect to clothes, and other things, which remain in cheſts, births, or cloſe places from the time they were ſhipped; and which, therefore, ſhould be ſubject to a longer or ſhorter quarantine, according to circumſtances.

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Flaxſeed and tobacco, the principal articles of import from America, being reckoned unſuſceptible matters, will not require purification; but ſome, ſuch as ſkins, &c. may require that proceſs, as well as the clothes and other things above notice. The moſt effectual meaſures for accompliſhing this ſhould be early purſued; and it is a great happineſs to find that it can be in general ſecurely effected by means of fire, ſmoke, and freſh air. *

The Quarantine Act, marking the great outlines only, leaves the detail and execution to the King in Council. But, conſidering the multiplicity of affairs claiming the time and attention of the Privy Council, and the important offices in the ſtate held by many of its members, it can hardly be expected that the board ſhould find leiſure ſufficient for entering into a variety of minute inquiries, abſolutely neceſſary for framing proviſional directions for a number of emergencies, incident to the ſuperintendence of quarantine.

But it is certain, that perſons acquainted with the effects of contagion, could ſoon deviſe proper expedients, which might be readily executed; and by which the neceſſity for performing the whole of the eſtabliſhed quarantine might be ſuper-

* See Obſervations on Contagion, Sect. II.

ſuperceded, trade be little incommoded, and the publick ſafety be effectually conſulted *.

REPORT of the COMMITTEE.

ON the ground of the moſt authentic information, the Committee have to ſtate to the inhabitants of this City and the neighbouring Country, that the diſtemper now raging in Philadelphia is moſt infectious in its nature, and moſt deadly in its effects. One hundred perſons at leaſt have uſually died in a day in that afflicted city, and when the laſt accounts came off, the number of thoſe who had fallen amounted to three thouſand!

To guard this country, thro' the Divine bleſſing, from this moſt calamitous contagion, the Lord Lieutenant and Privy Council have iſſued a proclamation, in which they have wiſely ordered, that veſſels coming from America ſhall perform quarantine — that is, that they ſhall, after their arrival, continue at anchor forty days without any communication with the land, and, in the mean time, the cargo ſhall be expoſed to the open air for the purpoſe of purification; and the

* The ignorance, rudeneſs, and avarice too generally attached to the inhabitants on the ſea coaſts, rendered it neceſſary to ſpeak in a higher tone of alarm and admonition in the Report, than appears in the tenour of theſe Conſiderations.

the law enjoins, that whoever refuſes to obey theſe orders, or tranſgreſſes the regulations of quarantine, by coming on ſhore during the forty days, ſhall be puniſhed with death.

But in vain will be all theſe precautions, in vain will be the wiſdom of the Law, or the vigilance of Government, to prevent the dreadful calamities which the introduction of this diſeaſe into the country muſt produce, if they are not zealouſly and actively ſeconded by the people themſelves. The performance of quarantine will avail nothing, if perſons from the land are ſo mad as to go on board the veſſels in queſtion, and ſtill leſs, if poſſible, ſhould any one be ſo deplorably wicked as to bring from thence any beds, bed-clothes, wearing apparel, linen, woollen, ſilk, or cotton, or any thing made of theſe, or any hair, ſkins, papers, rags, feathers, ſtraw, or hay; for, in any of theſe articles might the infection lie concealed, and by their being brought on ſhore, be communicated to the country, even tho' there was no appearance of ſickneſs in the veſſel; nay, the very perſons of thoſe on board might be a means of conveying the diſtemper to others, altho' by ſome accidental peculiarity of conſtitution they eſcaped it themſelves.

We therefore earneſtly entreat our neighbours and countrymen, to conſider and recollect, that

the only means, under Divine Providence, of ſecuring this country from this terrible ſcourge, will be to abſtain from every ſort of communication with either ſhips, perſons, or goods, until quarantine has been performed, and thoſe airings and fumigations put in practice, which are known in ſuch caſes to be effectual.

We beſeech you, for the ſake of your country, of your families, and of yourſelves, that you will not ſuffer either your folly, your obſtinacy, or your avarice, to betray you into an act of wickedneſs, compared with which common murder would be innocent.—Picture to yourſelves the miſeries of Philadelphia, lately ſo fair and flouriſhing, now the moſt pitiable city under Heaven —think of, not only houſes, but whole ſtreets ſhut up, and the wretched inmates left to ſink under a load of woes which the imagination cannot reckon, without even the ſlight conſolation of a pitying friend, for, alas, humanity itſelf ceaſes when humanity to another would be fatality to oneſelf!—Even eſcape itſelf is dreadful, when dear relatives and dearer friends, are left behind to periſh; and the unhappy fugitives themſelves know not but they carry along with them the very enemy which they flee from, and that the very next moment the chilling hand of Death may be upon them!—But even this ſtate of things, melancholy as it is, has had its alle-

viations,

viations, for *there* they could flee; there they had a vaſt extent of country before them, over which they might ſpread themſelves without hindrance, and where, if they eſcaped the contagion of the moment, they might reaſonably hope for future ſafety; and beſides, where towns and diſtricts are ſeparated from each other by large tracts of uninhabited land (as is the caſe in America) it may be expected, that, with proper care, the evil might be confined to the ſpot where it had begun — But *here*, the caſe would be far otherwiſe—*here* there would be no poſſibility of flight; the inhabitants of one town or county, would, at the riſk of their lives, reſiſt the entrance of thoſe of another town or county, where there could be the leaſt ſuſpicion of contagion—and what would even this deſperate reſource avail? Since in a country ſo thickly inhabited as this is, it would be next to impoſſible to prevent the ſpread of infection when once communicated. But theſe miſeries are neither to be deſcribed nor conceived—Let us be thankful to the Divine Goodneſs, that they are not actually upon us; nay more, that it is in our own power to prevent them, for this dreadful peſtilence is only communicable to this country by infection, and, with the bleſſing of God, there is no fear of infection, if the above direction be ſteadily, punctually, and univerſally obeyed.

OBSERVATIONS ON CONTAGION. *

SECTION I.

THE late melancholy accounts of the peſtilential fever at Philadelphia, muſt excite in perſons, ſituated as we are, a degree of awful apprehenſion and anxious inquiry; and this uneaſy ſtate of mind will be aggravated by the general unacquaintance with the nature of contagion.—To diminiſh ſuch inquietude, and to promote the publick ſafety, ſome information concerning infectious Fevers appears at this juncture particularly neceſſary.

It is a vulgar errour to imagine an infectious fever to be a calamity which extends itſelf to every perſon who approaches its ſphere of action. The proper definition of it is, "a diſtemper, which, in certain circumſtances, is, or may be, communicated to one, two, or more perſons, and that generally in its identical form and peculiar nature."

The effects of the communicated miſchief from bad fevers are often very rapid, and, in ſome

* Theſe Obſervations firſt appeared in the London-Derry Journal, ſoon after the meeting of the Citizens.

ſome inſtances, perceptible immediately after the poiſon is received; but it is certain, that the contagion may be dormant for a conſiderable time even in the body of the infected perſon, and more certainly in his wearing apparel, or other moveables, from whence it may at length come into action with irreſiſtible violence. This was the caſe, in the year 1758, when the *Yellow Fever* of a milder nature than that under conſideration, was brought from North America to Portſmouth. On that occaſion, ſeveral perſons were ſeized with the diſeaſe after they had been ſome time on land. And it was remarkable, that the fever, then imported from America, was much more malignant than any other that had appeared during the ſeaſon. Hence it is manifeſt, that we ought not to be lulled into ſecurity from the apparent healthineſs of the ſailors and paſſengers; for altho' all the people on ſhip-board may not only appear well, but be actually ſo, yet the articles in their cheſts, together with their bedding, which have not been expoſed to the air during the voyage, may be ſtrongly impregnated with the ſeeds of the diſeaſe. This is what the Faculty call the *Fomes*, which has always been obſerved to communicate a more virulent diſtemper, than the newly emitted vapour from the ſick.

Together with beds, bed-clothes, and wearing apparel, other articles, ſuch as buck-ſkins and nan-

hankeens, are matters very ſuſceptible of contagious taints. A ſmall parcel of any of theſe will be capable of conveying the contagion; for it is pretty certain, that a ſailor's bundle communicated one of the moſt deſperate plagues that ever deſolated a country, namely, the great plague at Marſeilles.

It is a miſtaken notion to conceive, that the ſtimulating effects of ſtrong liquors will ſecure a perſon from the impreſſion of contagion; for it has frequently happened, that a trifling exceſs in this way, has rendered a perſon not only more liable to infection, but, if infected, has made the diſeaſe more violent. This conſideration ought to prevent fiſhermen, or other perſons on the coaſt, from going out to meet American veſſels and boarding them at ſea, under the deluſive hopes of being protected by the power of *whiſkey*. Add to this, that if it be diſcovered that they have been on board ſuch veſſel, they will be compelled to perform quarantine, or be ſubject to all the grievous penalties in caſe of reſiſtance.

Another ill-founded opinion, which at this time may probably be entertained, is, that if an infectious malady diſappears, or even abates, in the place where it had raged, the danger of contagion from thence is either much leſſened, or

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totally removed. From what has been already obſerved, it may be ſeen, that no opinion can be more erroneous or deluſive. But let it be further conſidered, that times of general diſeaſe are alſo times of general plunder—that the articles moſt likely to be purloined, are thoſe which would be moſt liable both to imbibe and to retain infection—that thoſe who were poſſeſſed of them, would naturally endeavour to part with them to perſons leaving the country—that in this caſe, neither the ſellers would be conſcientious enough, nor the purchaſers knowing enough, to uſe means of preventing miſchief—and, conſequently, the period of declining or ſuppreſſed infection in Philadelphia would be actually the time of greateſt riſk to us, becauſe it would be then that this dangerous traffick would moſt probably be at its height. And, let it be alſo obſerved, that the ſubſiding of the diſtemper would naturally produce a general inattention to thoſe cautionary meaſures, which had been regarded in the ſeaſon of extreme danger, and, of courſe, increaſe, rather than leſſen, the hazard of communicating the contagion to other countries by infected articles.

It has likewiſe been ſuppoſed, that, becauſe cold weather has been known to ſtop the progreſs of an infectious malady where it has been prevalent, the ſame kind of weather will prevent its

its riſe in another country. But this, like the two former, is a very miſtaken notion; for the *Yellow Fever*, before noticed, which was imported into England, was not ſtopped in its commencement, nor obſtructed in its courſe, even by the cold of a very froſty December.

A fact, which ſhould make all the people of Ireland peculiarly circumſpect, is, that in this kingdom it has been obſerved from very early times, that the air of autumn is moſt favourable to the riſe and progreſs of the common fever of the country, and which is apt to continue more or leſs during the winter—conſequently it may be inferred, that the air of this ſeaſon of the year might be particularly fitted to foſter and extend a febrile contagion of whatever kind, and of whatſoever origin.

SECTION II.

IN whatever materials the infectious venom lurks, both cleanlineſs and ventilation, *ſometimes*, will not avail, either in repreſſing or moderating its activity. If, however, theſe meaſures fail on ſome occaſions, it ſhould not hinder any perſon from uſing them on every occaſion; for, their failure being rare, it is certain that they are in general highly conducive, during theſe calamities, to the preſervation of the healthy, and the recovery of the diſeaſed.

But if ſuch powerful auxiliaries, as cleanlineſs and freſh air, do *ſometimes* fail in abating or annihilating the ſecret ſource of contagion, it is a happy circumſtance, that a proper application of *Fire* and *Smoke* has been found ſucceſsful in purifying all tainted places, materials, and ſubſtances. To effect this, different methods have been propoſed; two of theſe we ſhall mention, which have been found effectual in ſhips, and might be eaſily applied to common apartments. The firſt is, by means of Tobacco: After carefully ſtopping all the openings, and every ſmall crevice of the ſhip, a quantity of tobacco is ſpread on ſeveral fires made with ſuch pieces of old rope as are called junk. Theſe fires being diſperſed into different places of the veſſel, their heat and ſmoke are afterwards cloſely confined below for a conſiderable time.—The ſecond method is, by Charcoal fires, ſtrewed with brimſtone. The heat and ſteam of theſe burning materials for this purpoſe, muſt likewiſe be long and cloſely ſhut up.

Firing ſmall parcels of gunpowder, and burning on hearths wood fires ſprinkled with brimſtone, or any agreeable aromatick, have been found uſeful in mending the air of tainted places. By ſuch means, a Society at Cheſter has been able to ſtop the ſpread of a very infectious

diſeaſe, the ſmall-pox, whenever it was obſerved to threaten that city.

The abuſe of fire, however, on ſuch occaſions, has cauſed its effects to be diſregarded by ſome, and to be ſuſpected of doing miſchief by others. The practice of burning large fires in the open air, in the ſtreets, and about the walls of towns, with a view either to exclude or to correct contagion, is founded on erroneous opinions; and has therefore not only failed in anſwering the intention, but has proved really hurtful, by diminiſhing the vital principle in the air, on which life and health eſſentially depend. For the ſame reaſon, *burning* any thing, ſuch as camphire, tar, ſpirits, &c. in the body of a chamber where the ſick are confined, or the ſound inhabit, is an improper expedient; but does not extend to the prohibition of burning aromaticks, &c. in fire-places, as before adviſed.

From the effects of *ſteam*, or the vapour of boiling water, in altering ſome kinds of bad air, it appears probable, that throwing a quantity of it into tainted apartments would contribute to ſweeten their air. For this reaſon, as well as for the ſake of cleanlineſs, waſhing ſuch places frequently with ſcalding water and mops, would be a very ſalutary practice.

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The ſpreading abroad of contaminated clothes to dry, or to be aired, without a previous fumigation of them, may be of dangerous conſequences. A damp air gives ſtrength to contagion, and a dry wind may ſpread the miſchief; and it was before obſerved, that contagion is not always checked nor ameliorated by cold weather. All ſuch ſuſpected ſubſtances ought therefore to be firſt fumigated in a cloſe place, in the manner above directed; and afterwards they may be ſpread abroad to air, or to undergo the *ſereine*, as it is termed.

The linen, and ſuch clothes as will admit of being waſhed, ought never at firſt to be put into hot water, as it is dangerous for any perſon to receive the ſteam that may ariſe from ſuch tainted articles in this ſituation. For however efficacious the pure vapour of boiling water may be in improving bad air; when ariſing from a *fomes*, it may become a vehicle of contagion, rather than an obſtruction againſt it. Such articles, therefore, ſhould firſt be ſteeped in cold water, in cold ſoap leys, in cold vinegar, or in lime water. — Where Lazarettos are eſtabliſhed, letters and papers preſented at the *barrier* of the *pratique* houſe, or office of health, are always dipped in vinegar before they are received.

It has been gravely aſked, Is it ſafe to uſe rain-water when a contagious diſorder prevails abroad or at home? Of all vulgar errours this is the moſt ridiculous; and ſerves to ſhow how neceſſary it is to inform the public mind on the ſubject, that it may not be led aſtray and diſtracted by ſuch flimſy doctrines. It is hard to ſay from what this idea took its riſe; unleſs it may have been from hearing, that, on the coaſt of Guinea, at the commencement of the rainy ſeaſon, which is there the moſt unhealthy, a diſagreeable ſmell is found in the air for ſome days. But how different is the climate of Africa from that of Ireland! And who could be ſo abſurd as to imagine, that a contagion would be tranſported in the air over the Atlantic, a boiſterous ocean 3000 miles broad—be collected in the clouds immediately over this kingdom—and thence poured down upon us in ſhowers of peſtilential rain! On the contrary, it is certain, that water, inſtead of being favourable to corrupted air, is a powerful opponent againſt it; eſpecially if the water be agitated. Hence the ſea air is ſo much more wholeſome than that of the land; and the air of the atmoſphere over the land is conſiderably improved by great rains.

Becauſe growing vegetables are known to abſorb impure air, it has been ſuppoſed that they become a *fomes* in tainted chambers, and hence gar-

garden-pots, containing any ſpecies of plants, are imagined to be pernicious in thoſe places. That growing vegetables draw in impure air, is undoubtedly true: but then it is a material part of their nouriſhment, and is entirely changed by the powers of vegitation; whilſt the air which they naturally emit, inſtead of being impure, is of the purer kind—conſequently flouriſhing plants are a great acquiſition in ſick rooms. They muſt, however, be placed in the ſun, or in the light, and ſhould not be ſuch as produce many ſtrong ſmelling flowers; for plants in the ſhade throw out bad air, and even the fragrant Roſe yields an unwholeſome exhalation.

SECTION III.

With Illuſtrations from an Account of the malignant Fever in Philadelphia. *

THE univerſal panick, the general deſpondency, the inſtances of diſtreſs, and the ſcenes of miſery, which afflicted the citizens of Philadelphia, have few parallels in the records of ſociety;

* The illuſtrations are taken from the 3d edition of "A ſhort Account of the malignant Fever in Philadelphia, by Mat. Carey," publiſhed 30th Nov. 1793.—No authority can be better than that of Mr. Carey, he having been one of the Committee of Diſtribution, moſt of the facts mentioned fell under his own obſervation, and thoſe of a different deſcription he collected from every perſon of credibility poſſeſſed of information.

ciety; and, whilſt they, even at this diſtance, excite our pity, they ſhould operate as warnings againſt ſupineneſs and negligence in ſuch formidable caſes, whether the danger appear remote, approaching, or at hand. For had the Philadelphians been more early on their guard, and known the ſource and diſpoſition of the venom, it is probable that the evil might have been checked in its progreſs, or at leaſt rendered leſs baneful in its effects. By cautionary meaſures, ſeaſonably purſued, the plague itſelf can be reſtrained and qualified. But before any proper notice was taken of the Yellow Fever, a conſiderable time, rather more than three weeks, was ſuffered to elapſe, during which a number of perſons were ſwept off by the diſeaſe.

It is true, when official notice was taken, the means preſcribed were of the beſt kind. The Mayor ordered the City Commiſſioners to have the ſtreets cleanſed, and the filth immediately drawn away. The College of Phyſicians alſo, in an addreſs to the People, recommended various precautions and rules. They adviſed great attention to cleanlineſs and freſh air — to avoid all unneceſſary intercourſe with the infected—to bury the dead very privately — to ceaſe tolling bells—to ſhun all fatigue of body and mind—to uſe fermented liquors, ſuch as wine, beer, and cyder, in moderation—and to accommodate the

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dreſs to the weather, but rather to exceed in warm than in cool clothing They likewiſe declared, that fires in the ſtreets were not only dangerous expedients, but ineffectual means of ſtopping the progreſs of the fever, and that they placed more dependence in the burning of gunpowder.

Doctrines ſimilar to theſe the reader may find laid down in our former ſections on Contagion; but we there differ from the American Faculty in ſome points. The College conſider the fumes of vinegar and camphire beneficial in infected rooms; whereas we diſapprove of thoſe expedients when uſed in the body of the ſick chamber, for the ſame reaſon that the College object to fires in the ſtreets, namely, *the deſtruction of the vital principle of the air*. Nor can we ſubſcribe to another opinion of the College, that theſe odoriferous articles cannot be too often uſed on handherchiefs and ſmelling-bottles, by perſons who attend the ſick. Camphire, tar, and other high ſcented ſubſtances (even the fragrant roſe) owe their ſmell to a certain volatile principle, chiefly compoſed of a ſpecies of air which is probably the vehicle, if not the foſterer, of contagious matter, and are conſequently very unfit means of prevention.

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But were theſe ſubſtances really qualified to ſecure againſt the entrance of infection by the noſe, we have an inſtance, in the caſe of the old grave-digger, where a total demolition of the ſenſe of ſmelling was no protection. This poor creature, relying on the vitiated ſtate of that ſenſe, thought himſelf invulnerable; and yet, in opening a recent grave, he took the infection by the mouth, and died.

Reſpecting the origin of the diſeaſe, a diverſity of opinion took place in Philadelphia. On the one ſide it was alledged, that it did not proceed from imported contagion, but originated from ſome putrid animal, or vegetable matters, particularly from a quantity of damaged coffee that had arrived from the Weſt Indies. On the other hand, it was maintained, that it muſt have been imported, becauſe its introduction could be traced from Weſt India veſſels, whoſe people had laboured under it; becauſe every ſtep in its progreſs teſtified a ſpecific contagion; and becauſe no caſe could be found on record, attended with ſymptoms which characteriſe the diſeaſe, that could fairly be aſcribed to damaged vegetables, nor any well authenticated inſtance of a fever ariſing from ſuch ſubſtances, and communicable from the ſick to the ſound.

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Numerous facts demonſtrate the very contagious nature of the Yellow Fever, and the danger of a *fomes*, or venom, lurking in tainted articles, as before ſtated in theſe obſervations. One inſtance is ſo much in point, that it deſerves particular notice: A negro ſervant, belonging to a Mr. Morgan, in New Jerſey, took up an infected bed floating on the Delaware, which ſpread the diſorder in the family, and Mrs. Morgan and her girl died of it.

The inefficacy of *whiſkey*, in ſecuring from infection as we formerly advanced, is confirmed by the great ſeverity of the fever on tipplers and drunkards. To thoſe, indeed, who lived in habits of intemperance, of any kind, it was remarkably deſtructive.

Amongſt the women, the mortality was far from being comparatively ſo great, as amongſt the men; nor among the old and infirm, as among the middle-aged and robuſt. From this lenity experienced by the female ſex, a uſeful inference may be drawn: That, on ſimilar occaſions, in performing the offices which they are peculiarly called upon to do for the ſick, they may diſcharge their duty with a certain degree of ſecurity, which will be further increaſed by the confidence that this ſuperior ſafety naturally inſpires.

At firſt, an idea prevailed in Philadelphia, ſupported by ſome obſervations made in Carolina, that the Negroes were not ſuſceptible of the infection; which afterwards appeared to be partly erroneous. For, although the Blacks were not nearly ſo liable to take the Fever, nor ſo ſubject to its fatal termination, as the Whites, they were not totally exempt either from its aſſault or its conqueſt. This peculiarity in the Blacks teſtifies a curious phyſical difference of conſtitution between them and the Whites, which perhaps we ſhall endeavour hereafter to explain. However, on that emergency, it anſwered this valuable purpoſe: The Negroes, being leſs apprehenſive, attended on the ſick with more cheerfulneſs, fidelity, and perſeverance.

Although the virulence of the infectious matter was ſtrong, it was not altogether irreſiſtible; for means were not wanting by which it was controlled and ſubdued. The jail of Philadelphia is under ſuch excellent regulations, that the diſorder made its appearance there only in one or two inſtances. Several circumſtances conſpired to produce this ſalutary effect. The priſoners are frequently cleanſed and purified by the uſe of the cold bath—they are kept conſtantly buſy at ſome work—in the jail-yard vegetation flouriſhes—and many of the people confined being employed in ſtone-cutting, the wa-

water, conſtantly running, kept the air in a moiſt ſtate, whilſt the inhabitants of the city were parched up by unceaſing heat. The ſucceſs attending this judicious mode of adminiſtration confirms ſeveral points that we advanced in our former ſections on this ſubject, namely, that the greateſt advantages reſult from a limited intercourſe of every kind, from attention to cleanlineſs, from the correcting property of growing vegetables, and from a watery atmoſphere, or rainy weather.

The bad conſequence proceeding from a want of cleanlineſs in particular, was but too plainly demonſtrated by the havock which the diſtemper made amongſt the poor, who kept dirty houſes, as whole families of them ſunk into one ſilent, undiſtinguiſhed grave. The mortality, likewiſe, in confined ſtreets, ſmall alleys, and cloſe houſes, debarred of a free circulation of air, greatly exceeds that in large ſtreets and well ventilated houſes.

The ſteps taken, in many places of the United States, to prevent the introduction of the malady, were very ſtrong; and, on other occaſions, might be deemed cruel. Goods carrying from Philadelphia, in a waggon, were burned; and a perſon accompanying them was tarred and feathered. Quarantines of different lengths, ex-

tending even to 40 days, were ſtrictly enjoined. A paſſenger-ſhip from this port, arriving in Auguſt with ſome of the people unwell, increaſed the alarm, and became the object of a rigid police. But all, even the harſheſt diſcipline, was juſtifiable on the ground of the firſt and ſupreme law of Nature, that of *ſelf-preſervation*. And, as no perſon can preſume that he, or his family, is not ſuſceptible of infection, it becomes the duty of all to concur in a ready and frank ſubmiſſion to ſuch regulations, as the civil power may judge neceſſary for the general ſafety. A good citizen will conſider the various inconveniences, attending theſe regulations, as a common misfortune, inſeparable from the condition of the times; and will comfort himſelf with reflecting on the happy conſequences that he has perceived to enſue from his concurrence, ſince he may have been a principal means of preventing vaſtly greater inconveniencies, of precluding the neceſſity of much more ſevere reſtraints, and perhaps of ſaving a flouriſhing, and favourite country from extreme miſery and deſolation.

Note—Theſe Obſervations are ſupported by the authorities of Lind, Prieſtley, and Ruſſell.

REMARKS ON DR. RUSH.

CHAP. I.

REMOTE CAUSES.

SECT. I.

Of Effluvia from putrefying Vegetable Subſtances, and from Marſhes.

THE diſtinguiſhed reputation of Dr. Ruſh as a medical teacher, a practitioner, and a writer, ſtamps his opinions with a currency, which, by many perſons, may be accounted preſumption to oppoſe, and perhaps, by ſome, perverſeneſs even to queſtion. But truth in Medicine, like light in Theology, cannot be explored by an eye dazzled with the luſtre of great *Names*, or too weak to examine an aſſemblage of *Tenets*; in the one ſcience, as well as in the other, it is neceſſary to hold in remembrance theſe excellent precepts, "Seek and

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ye ſhall find: knock and it ſhall be opened unto you." *

A gentleman, ſuch as the author of the work under conſideration, liberalized by literary attainments and purſuits, cannot take umbrage at Remarks ſpringing from motives of this nature; eſpecially if thoſe remarks be conducted with that candor and decorum, which belong to philoſophical inquiry, and againſt the laws of which, I truſt, I ſhall never be found a treſpaſſer. I therefore proceed to conſider, 1ſt, Dr. Ruſh's arguments concerning the remote cauſes, particularly as refering to the introduction of the fever into Philadelphia; 2dly, his theory of the proximate cauſe; and, 3dly, his method of treating the diſeaſe.—In the courſe of the ſurvey, we may meet with other points deſerving notice; but theſe appear to claim our firſt and principal conſideration.

After

* A ſimile, ſomewhat reſembling the beginning of the above, is uſed by Dr. Ruſh in the preface to his 2d volume of Medical Inquiries and Obſervations, which came to hand ſince the above was written, and in which I have the ſatisfaction of obſerving, that the Doctor concurs with me in the Theory and Practice which I maintain reſpecting the Internal Dropſy of the Brain, viz. That the effuſion of water ſhould be conſidered only as the effect of a primary inflammation, or congeſtion of the blood, in that organ; that the diſorder is chiefly confined to children; that it is connected with a ſymptomatick febrile ſtate of the ſyſtem; that bleeding and purging, uſed early, are the means moſt to be relied on; and that he did not experience the expected advantages from mercury.—The theory of inflammatory cauſe was before the publick for a long time antecedent to the hints, which he mentioned to his colleague, Dr. Wiſtar, on the ſubject.

After ſtating a conſultation which he held with Drs. Foulke and Hodge, on the caſe of Mrs. Le Maigre, in Water-ſtreet, our author writes thus: "The origin of this fever was diſcovered to me at the ſame time, from the account which Dr. Foulke gave me of a quantity of damaged coffee which had been thrown upon Mr. Ball's wharf, and in the adjoining dock, on the 24th of July, nearly in a line with Mrs. Le Maigre's houſe, and which had putrefied there to the great annoyance of the whole neighbourhood." * From this intelligence, he conſidered himſelf enabled to trace the diſeaſe to its ſource; and ſix or ſeven inſtances, which he had before ſeen, he imputes to the exhalations from this damaged coffee.

Convinced that the diſeaſe originated from this putrefying ſubſtance, he ſupports the opinion by ſetting forth, that "it is no new thing for the effluvia of putrid vegetables to produce malignant fevers. Cabbage, onions, black pepper, and even the mild potatoe, when in a ſtate of putrefaction, have all been the remote cauſes of malignant fevers. The noxious quality of the effluvia from mill-ponds, is derived wholly from a mixture of the putrefied leaves and bark of trees with water." †

To corroborate this doctrine, he adduces the authorities of Hillary, Lind, Cleghorn, &c. by whom he

* Ruſh on the Yellow Fever, p. 11, 12.—† P. 25.

he thinks it is clearly ſhown, that "the yellow fever in the Weſt Indies, and in all other countries where it is endemic, is the offspring of vegetable putrefaction. Heat, exerciſe, and intemperance in drinking, *diſpoſe* to this fever in hot climates, but they do not produce it, without the concurrence of a remote cauſe. This remote cauſe exiſts, at all times, in ſome ſpots of the iſlands, but in other parts of even the ſame iſland, where there are no marſh exhalations, the diſeaſe is unknown."*

The damaged coffee, he repeats, was expoſed at a time, and in a ſituation, which favoured its putrefaction and exhalation. "Its ſmell (continues our Author) was highly putrid and offenſive, inſomuch that the inhabitants of the houſes in Water and Front-ſtreets, who were near it, were obliged in the hotteſt weather to exclude it, by ſhutting their doors and windows. Even perſons, who only walked along thoſe ſtreets, complained of an intolerable fætor, which, upon enquiring, was conſtantly traced to the putrid coffee.

"The rapid progreſs of the fever through Water-ſtreet, and the courſe through which it travelled into other parts of the city, afford a ſtrong evidence that it was firſt propagated *chiefly* by exhalation from the putrid coffee. It is remarkable, that it paſſed firſt through thoſe alleys and ſtreets which

* Ruſh on the Yellow Fever, p. 147, 148.

which were in the courſe of the winds that blew acroſs the dock and wharf where the coffee lay, and that perſons were affected at a much greater diſtance from Water-ſtreet by that means, than was afterwards known by means of the contagion which was generated by infected perſons." *

Several authors of conſiderable credit, both ancient and modern, have maintained with Dr. Ruſh, that malignant diſtempers are cauſed by the effluvia from corrupted vegetables; and this opinion they adopted from a belief, that theſe kind of effluvia, acting as a ferment, produced a putreſcent ſtate in the fluids of the human body. But it appears from experiment, that the vapour of putrefying vegetables, inſtead of being a ſeptick, is a ſtrong antiſeptick matter, and preſerves animal ſubſtances by the fixed air which it contains †.

It has, indeed, been properly remarked, that ridiculous, if not dangerous, doctrines have been proclaimed, founded on deductions from ſome of theſe experiments, namely, That, in the time of a prevailing peſtilence, dead animals ſhould be thrown into the ſtreets and roads, to fill the atmoſphere with a putrid ſmell; and that in an epidemick ſeaſon, nothing is better or more ſalutary,

 than

* Ruſh on the Yellow Fever, p. 153, 156.

† Alexander's Experimental Enquiry, chap. v.—Dobſon's Medical Commentary on Fixed Air, ſect. v.

than for every one to ſmell, three times a day, either a neceſſary-houſe or a ſheep-houſe. *

The effluvium, which exhales from a maſs of putrefying vegetables, conſiſts in a mixture of fixed air, inflammable air, vital air, and phlogiſticated air. To this mixture of various airs, ſome part of the principles of the plant, namely, the oil and the acid, are ſuperadded; being not totally decompoſed, altho' the vegetable itſelf be ſo, but merely volatilized by the putrefactive proceſs. Inflammable air abounds in thoſe places where vegetables are rotting in heaps; and marſhes furniſh it in great quantity, from whence it has been characteriſed by the name of *marſh inflammable air*; but, in the latter caſe, it is extremely liable to decompoſition, and to paſs into the ſtate of phlogiſticated air.

This ready decompoſition of inflammable air is particularly obſervable in the diminution of common air by phlogiſtick proceſſes, in which a true inflammable air is firſt produced, and in its *naſcent* ſtate, as Prieſtley terms it, is immediately decompoſed, previous to the phlogiſtication of common air—But for the manner in which this is accompliſhed,

* Alexander's Experimental Enquiry, chap. vii.—Although putrid matter, as Dr. Alexander ſhows, will preſerve other ſubſtances from putrefaction, and although the air proceeding from ſuch matter be noxious to the lungs (Prieſtley on Air, vol, 2, p. 254, et ſeq.), yet I do not admit that it is the parent of pure fever.

pliſhed, I muſt refer the reader to Prieſtley's Works on Air, vol. ii. p. 266, and the ſucceeding pages; and in the 1ſt vol. p. 127, it is ſhown, that the volatile effluvium emitted from putrefying ſubſtances is phlogiſton, loaded with that matter which contributes to affect the noſtrils with the ſenſe of ſmell. Hence we may conclude, that the exhalations from putrid vegetables, and from marſhes, which may be ſuppoſed to injure the atmoſphere, and extend their influence to any diſtance, conſiſt principally of phlogiſticated air.

Beſides this product of plants in general, certain kinds yield, during putrefaction, a particular ſort of air, of a very fœtid ſmell, reſembling the hepatick odour. This ſpecies of vegetables is found to contain ſulphur and volatile alkali, which uniting, form a ſulphureous ammoniacal air, inflammable and highly offenſive; furniſhed plentifully by onions, radiſhes, &c. and ſtill more abundantly by cabbages. *

The obſervation of Lanciſi, that ſteeped flax and hemp are productive of what he calls a camp-fever, has been copied by many writers as well as Dr. Ruſh. It is an obſervation, however, which ſhould be received with caution. For although the effluvia are extremely offenſive, yet in this

 country,

* De La Metherie ſur l'Air Pur, &c. vol. ii. p. 150, 51, 52. —Prieſtley on Air, vol. i. part i. ſect. iv.

country, where prodigious quantities of flax are watered annually, and in warm weather, I never could trace our fevers to this ſource. The typhoid fever, it is true, to which this kingdom is liable, appears to be moſt prone to commence about the ſame time of the year. But the flax-water cannot reaſonably be accuſed of procreating the diſeaſe; becauſe, many years ago, when the culture of flax was little known in Ireland, the fever was obſerved to ariſe about the ſame ſeaſon. *

In Germany, people ſeem to concern themſelves very little about the danger ſuppoſed to ariſe from the effluvia of ſteeped flax. Nor do I find, as far as I have been able to ſearch on the point, that this kind of effluvia has been charged with generating malignant diſeaſes in Ægypt, a country early noted for its great cultivation of flax, and remarkable for a hot climate. On the contrary, the peſtilential diſeaſes, to which Ægypt is obnoxious, have been imputed to other cauſes, namely, to the annual inundation of the Nile, and the hot winds from the deſerts. †

As

* Boate's deſcription of the fever, which was common in Ireland in his day, vulgarly called the IRISH AGUE, correſponds with the leading features of the endemick fever of the preſent time; and his account of the moſt ſucceſsful method of treatment, is nearly the ſame as that on which, with us, the moſt dependence is in general to be placed.—See Boate's Natural Hiſtory of Ireland, chap. xxiv. p. 99.

† Lind on Diſeaſes, p. 39.

As to the celebrated inſtance of putrid cabbages related by Rogers, by which ſo many writers have ſupported their hypotheſes, it ſhould be conſidered, that when a malignant fever breaks out in a ſeminary, work-houſe, or manufactory, the perſons intereſted are at great pains to find out ſome pretence for fixing its origin on another cauſe than that to which it is really due; and generally pitch upon ſomething known to have emitted a diſagreeable ſmell in the neighbourhood.

Another ſource of illuſion is, that in our endeavours to account for epidemical diſorders, we are apt to lay hold of every perceptible difference which we diſcover in the air, or ſituation of places that are attacked, from thoſe that are free; and often, without duly examining and conſidering theſe diverſities, conclude that we have detected the cauſe of a diſeaſe, which is perhaps involved in inextricable difficulty.

"Thus," ſays a diligent inquirer, "if an epidemick diſtemper rages in a camp or village, and this camp or village be in the neighbourhood of a fœtid marſh, this marſh, as being moſt obviouſly different from any thing obſervable near the place, is immediately fixed upon as the cauſe of the diſtemper." By this, however, it is not intended to maintain, that the effluvia of a putrid marſh may not be unwholeſome, and occaſion certain diſeaſes,

or

or morbid prediſpoſitions; but, that theſe effluvia are not the direct cauſe of thoſe malignant fevers, for which they have been ſo often blamed. Beſides, the inſalubrity of a marſh, particularly in this reſpect, may be doubted, if we conſider that, when an army encamped in its vicinity continues healthy, no notice is ever taken of ſuch a thing having been there. Whereas, when ſickneſs begins to prevail, if any marſh be near, it is immediately charged with being the cauſe of the evil. *

The Engliſh troops, when at Breda, although there were no marſhes in the neighbourhood, were very ſickly; and in other places, where they were collected in barracks, many infectious fevers, &c. prevailed. This has been aſcribed to damp ſituations; but in the preſent inſtance, the water was two or three feet from the ſurface, and could not be reckoned a direct agent in the buſineſs. The fevers which occurred there appeared to be owing to the effects of a number of men (ſome, perhaps, with a febrile taint) ſleeping, &c. in the ſame apartment, rather than to aqueous vapour; for thoſe who were lodged in private houſes, being in ſmaller groupes, continued quite healthy.

It has been very properly aſked, if marſh miaſmata have ſuch a power in bringing fevers, dyſ-

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* Alexander's Experimental Enquiry, chap. viii. p. 63, 4, 5 & ſeq.

enteries, or even agues, upon thoſe people who live, or are encamped in their neighbourhood, why are not the labourers, who are employed in draining bogs and marſhes, alſo affected with theſe diſeaſes? In Ireland, which abounds with thoſe kind of places, and in which the peaſantry work a great deal, I do not find that fevers or fluxes appear ſooner or more frequently amongſt the labourers, than amongſt thoſe who are never engaged in ſuch work.

Dr. Ruſh himſelf furniſhes ſome obſervations, which ſtrengthen this remark: The firſt ſettlers in America received the lands, from the hands of Nature, pure and healthy. Even the ſwamps in that continent are healthy before they are cleared. "In the Delaware State," ſays the Doctor, "labourers paſs whole ſummers and autumns in making ſhingles in cedar ſwamps, without being affected by fevers or ſickneſs of any kind. Perſons who ſpend the warm months in the cedar and pine ſwamps in North Carolina, are likewiſe equally free from diſeaſes." *

Nor does it appear, that, in the Weſt Indies, the the utmoſt care in clearing and draining ſwampy grounds, nor the ſetting in of cool winds, will prevent the recurrence of the Yellow Fever amongſt troops. For we find from Mr. Chiſholm (who is quoted

* Medical Inquiries and Obſervations, vol. ii. p. 266, 67.

quoted by Dr. Ruſh) that, although the beſt means were uſed, yet they did not preclude a fever of this kind, which began in Grenada in September, from returning the December following, with all its formidable and deſtructive ſymptoms. Neither does it appear, that inundations producing marſhes ought to be accuſed of exciting the Yellow Fever; becauſe Dr. Balfour's obſervations ſhow us, that this diſorder occurs at Banaris and other places in Bengal, not leſs than three hundred miles diſtant from the reach of tides.

Theſe are confirmed by other obſervations: We find, from the 9th volume of the Tranſactions of the Batavian Society, that a fever, which carried off nearly one ſeventh part of the inhabitants, prevailed in Rotterdam, after draining ſome marſhes in the nighbourhood of that city. And Dr. Jackſon ſays, that the fever at Savanna la Mar, in Jamaica, which is ſituated in a level country, and partially ſurrounded by a moraſs, was more mild in its ſymptoms, than in moſt other parts of the iſland. He, therefore, thinks that the neighbourhood of ſalt marſhes is not ſo prejudicial to health as has been imagined; on the contrary, he believes it to be a healthy ſituation, eſpecially reſpecting its diſpoſition to generate febrile action. *

Nay,

* Edinburgh Med. Com. vol. xviii. p. 271.—Analyt. Review, vol. x. p. 347. Id. vol. xi. p. 145.

Nay, the influence of marſh miaſma is ſo dubious, that it has been accounted an imaginary being, becauſe intermittents, which are reckoned the peculiar offspring of fenny places, very often appear where this miaſma cannot well be ſuppoſed preſent. Were the putrid effluvium from any fluid capable of cauſing fever, ſurely the *bilge-water* in ſhips would be equal to this effect. Yet it is a certain fact, that the men in many veſſels, in the Navy, have continued in perfect health, notwithſtanding the bilge-water in the hold was very offenſive, not only ſo, but, when taken into the ſtomach, would prove in a manner poiſonous. *

I cannot indeed diſcover ſufficient grounds for believing, that the effluvia from putrid vegetable ſubſtances, from marſhes, and from ſtagnant water, are the ſole exciting cauſes of pure fever; but there are good reaſons for preſuming, that they inherit a quality which ſerves as a nidus for contagion, reſembling, as it were, the office of thoſe kinds of matter that are known to be adapted to the nurture of animal ova—harbouring them, foſtering them, and giving them activity.

This nidus appears to be phlogiſticated air, which we have ſhewn to be a product of putrid vegetable and marſh effluvia, and which is a combina-

 tion

* Beddoes's Obſervations on Calculus, &c. p. 263. — Lind on Fevers and Infection, p. 113.

tion of pure and inflammable air. That phlogiſticated air is the natural vehicle of contagious matter, is rendered very probable from this air being expelled by the lungs and by the ſkin. Its expulſion from the lungs is generally admitted. And ſome late anatomical and phyſiological reſearches tend to prove, that there is a ſyſtem of veſſels, which ſecrete a fluid exhaled through the ſkin, ſimilar to what proceeds from the lungs, although the ſyſtem of veſſels in the former is different from the air veſſels of the latter. Indeed, from the experiments of Count de Milly, in conjunction with M. Lavoiſier, it ſeems demonſtrated, that the emanation from the ſkin is a gaſeous fluid, of the ſame nature as pulmonary air. *

Many facts might be adduced in ſupport of this opinion; at preſent I ſhall mention only one: "I have known," ſays Dr. Ruſh, "ſeveral inſtances of families being preſerved from fevers by an accidental copſe of wood ſtanding between a mill-pond and a dwelling-houſe, and that in caſes too where the houſe derived no advantage from a high ſituation. The trees *around* or *near* a mill-pond, act perhaps in a ſmall degree *mechanically*. By ſheltering the pond from the action of the ſun, they leſſen exhalation, as well as obſtruct the paſſage of the vapors that are raiſed, to the adjacent parts. But

* Edinb. Med. Comment. vol. x. p. 112.—This will appear more fully in the ſequel.

But they act likewise *chemically*. It has been demonstrated, that trees absorb unhealthy air, and discharge it in a highly purified state in the form of what is now called dephlogisticated air." * This explains why cutting down the woods, or what is called *clearing* lands in America, contributes to render that country unhealthy; for it appears, that, as the *draining* of the swamps, &c. does not keep pace with the *felling* of the timber, the matter for generating phlogisticated air is augmented, and the means of purification are diminished.

SECT. II.

Of the Effluvia from putrefying animal Substances.

IF Dr. Rush be earnest in imputing the generation of fever to the exhalations from putrefying vegetables, he is on the other hand disposed to think, "it is much less common for the effluvia of putrid animal matters to produce fevers. How seldom do we hear of them in the neighbourhood of slaughter-houses, or of the work-shops of skinners or curriers?" †

Together with the several kinds of air which the exhalation from putrid vegetable matter contains,

 that

* Medical Inquir. & Observ. vol. ii. p. 269, 70.

† Account of the Yellow Fever, p. 25, 26.

that from animal putrefaction is compoſed of a ſpecies of elaſtick fluids, called ammoniacal air and ſulphureous ammoniacal air. Theſe airs are inflammable and very fœtid; and phlogiſticate the atmoſphere at leaſt equally as the effluvium from vegetable putrefaction. They alſo contain certain volatilized ſubſtances, denominated oily, which modify their influence, according to the nature of the matter from whence they proceed. And although they are active, and emit a conſiderable ſtench, yet I ſo far concur with Dr. Ruſh, that I cannot aſſent to the doctrine of ſome writers, who conceive, that they conſtitute the miaſmata which generate contagious diſeaſes. * Like putrid vegetable effluvia, I believe they are proper vehicles of contagion; and, therefore, are not principles, but acceſſories only. Or, in the language of the Syſtematick, to whom Dr. Ruſh ſeems attached, every impurity of air diminiſhes its ſtimulus; and conſequently impure air, or air blended with impure matters, without doubt debilitates, and produces aſthenick diatheſis, which prepares the body for the operation of contagion.

That ſlaughter-houſes yield a conſiderable quantity of putrid animal effluvium, is certain; eſpecially in places circumſtanced as Cork is, in this king-

* "Ils ont la plus grande influence ſur l'economie animale, et conſtituent les differens miaſmes que la medecine reconnoit pour cauſe des maladies contagieuſes." De La Metherie ſur L'Air pur, &c. tome ii. p. 155, 66.

kingdom. It is built on iſlands ſituated in a deep valley, and ſurrounded by ſeveral branches of the river Lee. Theſe iſlands were naturally marſhy, and ſubject to be overflowed by ſpring tides, until the ground was raiſed by art. The reſt of the marſhes, both on the eaſt and weſt, are periodically covered by thoſe tides; and, in the intervals, putrid vapours are exhaling from them. Continued ridges of hills, on which the ſuburbs are built, bound this vale on the north and ſouth.

In the north and ſouth ſuburbs, the ſlaughter-houſes are numerous; the blood and ordure from which are received in vaſt pits, where they are ſuffered to putrefy, and emit ſuch noiſome ſteams as corrupt even the moſt wholeſome breezes that paſs over the city; and, upon great rains, theſe ceſs-pools diſcharge, by the declivities of the hills, their fœtid contents into the river. Nor is this ſource of impure air confined to theſe places; for great quantities of filth and offals croud the ſtreets, and particularly the cloſe confined alleys and lanes where that deſcription of people reſide, whoſe circumſtances are peculiarly adapted to foſter and propagate febrile contagion. *

That we ſeldom hear of fevers being particularly prevalent in the neighbourhood of the work-ſhops of ſkinners and curriers, and, I would add, in that of

* Rogers on Epidemick Diſeaſes, p. 36, 37.

of tan-yards, is not at all ſurpriſing. The proceſſes, which the hides undergo in the hands of the tanner, prevent the putrid fermentation; and, conſequently, the generation of that kind of effluvium favourable to the action of contagious miaſmata, is primarily oppoſed. The ſmell iſſuing from tan-yards (which, by the bye, very much reſembles that from ſteeped flax) however offenſive, ſeems to be very different from that which is emitted from putrefying animal ſubſtances; and probably is owing to principles which do not adequately phloghiſticate the air. If this be the caſe with reſpect to tan-yards, certainly the work-ſhops of thoſe tradesmen, ſkinners and curriers, who receive the ſkins almoſt entirely freed from the corruptible parts, will be ſtill leſs chargeable with being the ſource of contaminating effluvia.

Several facts tend to prove, that very foul animal effluvia will not excite fever, without the concurrence of ſpecifick contagion. Two men travelling on foot, in the county of Kent, were aſſaulted near a town on the road with an extremely loathſome ſtench, which they imagined aroſe from animal putrefaction, and which continued to offend them for the ſpace of two hours. The ſky was clear without the leaſt cloud; and, from the direction of the wind, this vapour could neither come from the unwholeſome air of Sheppey iſland, nor from the muddy banks of the river Thames or the

Med-

Medway, nor from the Rhumney marſhes. Yet theſe men neither grew ſick at the time, nor experienced any future inconvenience from this fœtid exhalation. *

At Calcutta, 146 people were put into a very confined place, well known by the name of the *black-hole*, about 6 o'clock p. m. and only 23 ſurvivors came out the following evening. Several of theſe had wounds; and, although the ſurvivors were expoſed to the ſtench of theſe wounds, and to that ariſing from the dead bodies of their companions, yet we do not hear of any of them having been affected with legitimate fever. Mr. Holwell, indeed, is reported to have had a fever, ſucceeded by a great number of boils; but it does not appear that he communicated the diſeaſe to any other perſon. Nor do we find, that any more of the ſurvivors experienced any febrile affection; which muſt have been the caſe, had the animal mephitick air alone been capable of creating ſuch diſtemper.

A number of patients, labouring under the ſcurvy, were crouded together in a cloſe apartment in a ſhip of war returning from the Weſt Indies, which rendered the place ſo diſagreeable and ſuffocating, that the Surgeon during his viſits could ſcarcely breathe in it, or remain with the ſick any length of time, without being obliged to have frequent

* Fuller on Eruptive Fevers, p. 97, 98.

quent recourſe to the freſh air upon deck, and ſometimes to the ſpirit of hartſhorn, or to a glaſs of wine, for his immediate relief; "yet out of above an hundred ſick ſent to the hoſpital by this ſurgeon, not one was remarked to have any ſymptom of a contagion generated in that apartment." *

The phyſician commiſſioned to viſit Newgate (as mentioned in the preface) found 250 men confined in the common ſide of the priſon in a few wards; and not many days before there had been 200 miſerable females crouded in a few rooms; yet not one of either laboured under a febrile complaint.

The offenſive ſmell, proceeding even from the excrementious diſcharges of patients confined with non-febrile diſeaſes, does not occaſion pure fever. In a bilious colick, the evacuations both upwards and downwards, have been ſo intolerably offenſive, as to produce ſtomach ſickneſs and retchings in one caſe, and giddineſs, vomiting, and tremor in another; yet fixed fever does not appear to have been the conſequence in either. †

Many inſtances have occurred, in which thouſands of dead bodies have been left to putrefy on the field of battle, without cauſing putrid fever; and it is well known, that in no caſe has the origin of this diſeaſe been traced to the effluvia of ſub-

* Lind on Fevers and Infection, p. 11, 12—† Id. p. 59.

ſubjects in a diſſecting room. Nor have fevers been obſerved to originate, or to rage more ſeverely, in houſes ſurrounding grave-yards, in the middle of large towns, though the ſtench of the bodies accumulated in ſuch receptacles, is often intolerably offenſive. "It is true, that the putrefaction of dead bodies generates a poiſon, which is highly noxious when received into the living body, by a wound, or any raw ſurface, but this poiſon does not ſeem to infect, like that of fevers, by exhalation, and its firſt effect (unlike the other) is to occaſion the death of the part where it is admitted." *

It is a queſtion, whether the dead body even of a perſon deſtroyed by the plague or fever, be capable of communicating the infection. In the plague, it has been ſuppoſed, that the dead body, while it continues warm, may infect, and that it is even more infectious than the living patient. On the other hand, it is averred, that bodies dead of the plague have been diſſected with perfect ſafety. As to fevers, if a part only of the animal fluids be rendered contagious (which moſt probably is the caſe) when the patient dies, the exhalation of the poiſon muſt be ſtopped by the extinction of the morbid action.

* Ferriar's Med. Hiſt. and Reflect. p. 231, 233.—Why are malignant fevers ſeldom, or never, generated in ſlave ſhips, where human effluvia muſt be in a very vitiated ſtate?

The putrid exhalations from living bodies in caſes of gangrene and ſphacelus, being due to the vital motion and heat, ceaſe after death. The ſigns of the entire abolition of life may be diſtinguiſhed by two ſtates; that of incipient, and that of complete, putrefaction. The chemical products of theſe two ſtates are very different—acidity being the mark of the firſt, and alkaleſcence of the ſecond. From the former ſtate, it is maintained, that no danger can ariſe to the living; and from the latter, it is ſuppoſed, that an unwholeſome vapour emanates, ſimilar, no doubt, to what eſcapes from all animal ſubſtances during the putrefactive proceſs. *

In the caſes which have been brought to ſhew, that infectious effluvia ariſe from putrefying bodies, it does not appear that they produced any ſymptoms reſembling thoſe of peſtilential fever; on the contrary, they acted by direct ſtimulus, exciting inflammatory complaints; from which it may be inferred, that they are eſſentially different from febrile contagion. †

* Memoirs of Science and Arts, vol. i. part i. p. 181.

† Ferriar's Med Hiſt. and Reflect. p. 233, 34.—The dreſſings of a ſmall accidental wound in Dr. Ruſh's finger (ſee on the Yellow Fever, p. 354) dropped off in the hurry of buſineſs, and, although the raw ſurface was conſtantly expoſed to the contagion in feeling pulſes, the wound inflamed but a little, and healed in a few days—Againſt this illuſtration, the Doctor's ſuppoſition, viz. that he was previouſly ſaturated with the infection, does not militate; becauſe, he muſt firſt prove, that fever may be inoculated, before he be juſtifiable in his concluſion.

I may now fairly conclude, that *pure fever* is not generated by marſh miaſma, by the effluvia from ſtagnant water, nor by the exhalations from putrid vegetable nor animal ſubſtances.

That the yellow fever at Philadelphia did not ariſe from the effluvium of the rotten coffee, is a concluſion verified by the judicious report of Drs. Currie * and Cathrall, who maintain, that "there is not one caſe to be found with the ſymptoms which characterize the diſeaſe under conſideration, that can be fairly aſcribed to that cauſe, nor any well authenticated inſtance of a fever occaſioned by damaged vegetables, communicable from the ſick to the ſound" † — an inference ſtrongly corroborated by the facts and reaſonings contained in the preceding ſection.

But that the diſeaſe was really imported from the Weſt Indies by means of three veſſels, the Amelia, Sans Culottes, and Flora, appears to be rendered almoſt certain from the judgment and accuracy with which the ingenious reporters, juſt mentioned, traced it to that ſource. It was at firſt confined to a few perſons in that part of Water-ſtreet contiguous to the ſuſpected veſſels, and from thence

* I regret that I have not yet been able to procure this reſpectable phyſician's "Treatiſe on the Synochus Icteroides, or Yellow Fever, as it lately appeared in the city of Philadelphia."

† Carey's Account of the Yellow Fever at Philadelphia in 1793, chap. ii.

thence was propagated to other parts of the city, proceeding with the greateſt violence through its proper channels — cloſe unventilated lanes and alleys. *

The College of Phyſicians, three only excepted, confirms this opinion. "No inſtance," ſay they, "has ever occurred of the diſeaſe called the *yellow fever*, having been generated in this city, or in any other parts of the United States, as far as we know; but there have been frequent inſtances of its having been imported, not only into this, but into other parts of North America, and prevailing there for a certain period of time; and from the riſe, progreſs, and nature of the malignant fever which began to prevail here about the beginning of laſt Auguſt, and extended itſelf gradually over a great part of the city, we are of opinion, that this diſeaſe was imported into Philadelphia by ſome veſſels arrived in the port after the middle of July."

This declaration of the College, if it required any ſupport, is materially ſtrengthened by the teſtimony even of one of the diſſentients, Dr. Redman, the preſident. For we are told by Mr. Carey, that from this phyſician's notes it appears, that, when the yellow fever occurred in Philadelphia, in 1762, it was introduced by a mariner, who arrived ill with it from the Havannah, and who, having taken

* Carey's Account, &c. chap. ii.

taken lodging, communicated the diſeaſe to the family where he reſided, from whence it ſpread by contagion to other parts of the city.

The citizens, therefore, (notwithſtanding what Dr. Ruſh and others of the faculty endeavoured to inculcate) having good grounds to believe, that, on the late occaſion, the malady was of foreign original, could not be actuated, as the Dr. alleges, by mere prejudice. Nor is this concluſion invalidated by the repreſentation of Dr. Hutchinſon, who being port-phyſician, and not having been the firſt to detect the entrance of the diſeaſe, would naturally be diſpoſed to aſcribe it to ſome indigenous cauſe.

Moreover, on the preſumption that the fever was engendered by the effluvia from either vegetable or animal ſubſtances putrefying upon the wharfs, how are we to explain its appearing in Kenſington, where thoſe miaſmata could not reach undiluted, about the ſame time that Dr. Ruſh was called to viſit Dr. Hodge's child near the ſuppoſed cauſe of infection?—On ſuch a preſumption, the cotemporary viſitation of the diſeaſe in Kenſington and Water-ſtreet is inexplicable. But on the ground of its being imported from the Weſt Indies, that occurrence may be accounted for in a ſatisfactory manner, by alleging that ſome of the ſailors, paſſengers, or articles from tainted veſſels made their

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way to Kenſington very ſoon after their arrival in port. The fact indeed is, that ſome Daniſh ſailors and other perſons, who had been in the neighbourhood of the infected places, carried the fever to that village;* and, that theſe people caught the diſeaſe by contagion, we muſt continue to believe, until we have better reaſons for aſcribing its origin to the damaged coffee, than thoſe we are yet furniſhed with on the affirmative ſide of the queſtion.

The ſame opinion is entertained by a well-known and juſtly celebrated profeſſor, Dr. Duncan, who is very converſant in medical inquiry, and who writes thus: "Indeed, when we conſider, that it is on all hands allowed, that the diſeaſe after making its appearance, was propagated only by contagion, it ſeems much more probable, that the contagion itſelf was imported, than that it originated in Philadelphia; eſpecially when we reflect, that at the time of its firſt appearance in that city, and for many months before, it had raged with great ſeverity in different Weſt India iſlands, from whence there were frequent arrivals at Philadelphia." †

Nay, further—In the hiſtories of the yellow fever, furniſhed by the beſt authors, we find included the ſame ſymptoms, as thoſe which Dr. Ruſh ac-

* See Dr. Ruſh on the Yellow Fever, p. 25.

† Med. Comment. vol. xix. p. 364.

accounts characteriſtick of the diſeaſe. * We find the ſlow pulſe, called by him the *ſulky*, and by one of his pupils, the *undeſcribable* pulſe — we find the indications of determination to the head—we find the bilious affections—we find coſtiveneſs—we find childhood not exempt — and we find the peculiar rapidity of progreſs. Can we, then, at all doubt the identity of the Philadelphia fever with that which prevails in the Weſt Indies? — And if the ſame, why might it not have been imported from theſe iſlands into that city?

SECT. III.

Of Contagion.

THE great difficulty, or in general the impoſſibility, of tracing the contagious ſtream to the particular ſpring from whence it iſſues, has cauſed much confuſion and contrariety of opinion reſpecting the operation of infectious effluvia. Dr. Ruſh, for inſtance, imputes the germination of the Philadelphia fever to exhalations from putrid vegetable or animal ſubſtances; whilſt he attributes its ſucceſſive production to the effects of contagion. But an infectious, or morbid effluvium, emitted from the

* The authors here alluded to are, Sir J. Pringle; the two Linds; Dr. Rouppe, whoſe narrative Lind reckons full, clear, and maſterly; Dr. Bruce, who was a native of Barbadoes, and whoſe account is original; Drs. Clark, Hunter, Blane, and Curtin.

the living body, muſt contain ſome elementary principle, different from any compoſing that which ariſes out of dead vegetable or animal matter. This being the caſe, to ſay that miaſma and contagion produce one and the ſame diſeaſe, would be ſaying that two diſſimilar cauſes, acting on the like ſubjects, bring forth a ſimilar effect; which would be alleging a phyſical abſurdity.

On the contrary, as there is good reaſon for ſuppoſing pure fever to be an idiopathick diſeaſe, engendered by its own proper cauſe, which cauſe muſt conſequently be uniform; and, as the epidemick under conſideration is evidently a pure fever; it follows, that it muſt have originated from an infectious *fomes* derived from a febrile ſyſtem. A different opinion leads to the diverſification of fevers, which has done infinite miſchief, particularly amongſt the troops in the Weſt Indies in the laſt war.* In this particular, we derive conſiderable ſupport from Dr. Ruſh's book, p. 40; for it is there maintained, that the proximate cauſe of the yellow fever, and of all other fevers, is the ſame. The identity of the proximate cauſe, then, being admitted, fever muſt conſequently be a primary diſeaſe of a ſpecifick nature. That the bilious remitting fever, in whatever part of the world it takes place, is radically the ſame, Dr. Gardner thinks mani-

* See Robertſon's Obſervations and Eſſay on Fevers; and Reide on the Diſeaſes of the Army.

manifeſt from numerous obſervations; and Dr. Cullen ſeems confident, that in every ſpecies of contagious fever, whether exanthematous or not, the contagion is eſſentially one matter. *

Although we can but ſeldom aſcertain the birth place of a ſpreading contagion, yet, as reſearches on this head have been ſometimes ſucceſsful, we may from thence preſume, that zeal and perſeverance, in our preſent ſtate of knowledge, would be attended with farther advantages. With reſpect to the plague, Sennertus avers, that whole families and towns have been infected with it, from the venom which had been pent up in clothes and beds for the ſpace of many years. The variolous contagion will remain latent even in linen during ſeveral weeks, and ſometimes for a number of months. Febrile contagion, which is leſs inveſtigated, and which is ſeemingly a more fugitive thing than either the peſtilential or variolous poiſon, has been known to reſide in a garment ſeveral weeks.

Contagion ſometimes will adhere with an extraordinary pertinacity even to ſeaſoned wood; † for although a ſickly veſſel has been well fumigated

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* Gardner on the Animal Œconomy, ſect. viii. p. 350 & ſeq. — Cullen's Firſt Lines, vol. i. chap. ii. parag. 83.—And Dr. Ruſh, p. 217, where we find Dr. Stevens, whoſe talents and experience are recorded in the ſame book, affirming that contagion is the native ſource of yellow fever.

† Ruſh on the Bilious Yellow Fever, p. 99.

with the vapour of tar, yet the ſeeds of a malignant fever have broke out after ſome time in the ſame veſſel, and the diſeaſe has been thence conveyed into others. And though all the ſick from the tainted veſſels were removed—though theſe veſſels and crews were kept remarkably well ventilated and clean — yet the infectious venom remained long lurking in the timber.

No place is better calculated for diſcovering the ſource and progreſs of contagion, than the navy. An infectious malady can be more diſtinctly traced in fleets, or in a number of different ſhips, than in towns and villages, "as all the ſhips which compoſe a ſquadron, are under the ſame influence of diet and climate, the circumſtances of the men being likewiſe in other reſpects for the moſt part ſimilar." Hence a contagion may often begin in a ſecret manner, and ſpread itſelf unſuſpected over a town or village; while in a fleet of ſhips its commencement and extenſion become more apparent, from its confinement to one or more veſſels.

From the retentive diſpoſition of goods and ſhips, few people have ſuffered oftener than the Philadelphians, by the importation of the contagion of the yellow fever from the Weſt Indies through thoſe mediums. Along with other inſtances on record, I beg leave to call the reader's attention to the following: "A gentleman dying at Barbadoes (ſome years

years before 1761) his wearing apparel and linen, packed up in a cheſt, were ſent to his friends at Philadelphia, where, upon opening the cheſt containing thoſe tainted veſtures, the family was taken ill; and the clothes being unluckily hung abroad to be aired, they preſently diffuſed the contagion of this *yellow fever* over that town; by which the gentleman, who furniſhes me with this relation," ſays Lind, " was an unhappy ſufferer, and of which fever two hundred perſons die." *

That the fomes of the yellow fever may remain in all its vigour for a conſiderable time, even in theſe iſlands, we have indubitable proof. Three months after this fever had entirely ceaſed in Haſlar hoſpital, two nurſes, lodging in the ſame chamber, were ſeized with fevers, and both became yellow; one died, the other recovered. By a ſtrict examination, it was diſcovered, that they had concealed ſome ſhirts and clothes belonging to infected men from America. †

 When

* The danger to which they are manifeſtly expoſed from this ſource of infection, has no doubt been the cauſe of eſtabliſhing the office of Port-phyſician. — Why do fevers ſometimes prevail in long voyages to hot climates, ſeeing that the perpetual winds betwixt the tropicks, called trade winds, have a conſiderable influence in preſerving the health of ſeamen, and that they are not expoſed to marſh miaſma, or vegetable exhalation?—This may be reaſonably accounted for by ſuppoſing, that contagion lay lurking in the timbers of the ſhip, clothes of the men, or other articles on board.

† Lind on Fevers and Infection, p. 2, 23, 36, 73, 74.

When a nurſe is ſeized, or when two or three perſons in a family are attacked with a fever, it is imputed ſolely to cold, fatigue, grief, or the like debilitating cauſes; which ſeem to act no otherwiſe, than by ſtrongly diſpoſing the conſtitution to receive and animate the taint. And when ſuch a diſeaſe appears in a boarding-ſchool of boys or girls, the calamity is often aſcribed to cauſes which are not in the leaſt acceſſory. Of this Dr. Lind relates an inſtance; and I have been able, more than once, to trace very virulent contagions to thoſe kind of places.

I do not, therefore, ſuppoſe with Dr. Alderſon, and others, that the mere phlogiſtication of the air by reſpiration, no more than its impregnation by marſh and other exhalations, will be a generative cauſe of pure fever. But with Dr. Alderſon I agree in aſcribing to a phlogiſticated ſtate of the air, the property of a nidus for contagious effluvia; and, in addition to that ſtate produced by reſpiration, I am led to conceive, as already noticed, that cutaneous ſecretion is alſo a phlogiſticating proceſs. Thus I ſhould imagine, that the contagious matter of fever, when expelling by the lungs or ſkin, is combined with phlogiſticated air; which, like liberated fire giving elaſticity to bodies in combination, may communicate to that matter increaſed diffuſibility, and, in certain caſes, virulence—as appears deducible from the effects of cleanlineſs

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and ventilation in ſubduing or moderating contagion.

As the phlogiſticated ſtate of the air depends upon the ſeparation of one of its conſtituent parts, whereby the remainder becomes capable of keeping ſuſpended the contagious matter; it has been conceived, that, if this ſtate of the air be reſtored to its former purity, by adding to it that portion of which it had been deprived by the pulmonary and cutaneous functions, its powers of elective attraction will be altered, and that which was before held ſuſpended, will now be precipitated. This precipitation of the morbid mephitis (conjectured to be in ſome caſes in the form of powder) being depoſited upon certain ſubſtances qualified for receiving it, is ſuppoſed to conſtitute what authors have agreed to call a *fomes*.

From ſome facts, however, it appears that the contagion is not ſoon, if it be at any time entirely, ſeparated from the phlogiſticated air. Upon viſiting the cells where ſome men were confined with contagious diſeaſes, Mr. Howard's clothes became ſo impregnated with the effluvium, that he could not bear to ride in his chaiſe, but was obliged to expoſe himſelf to the open air on horſeback; and the vinegar, to which he was always accuſtomed to ſmell, became likewiſe ſo ſaturated as to be intolerably

erably offenſive. * An atmoſphere charged with a peculiar odour, well known to thoſe converſant in the ſmall-pox, ſurrounds patients in that diſeaſe, even before the eruption of the puſtles; and clothes, which are long retained about the ſick, preſerve the ſame odour a conſiderable time.

This theory enables us to account for a number of facts—why combuſtion injures the air in an epidemick conſtitution—why the upper ſtrata of air in a room are pure, while the lower are vitiated—why ventilation is not of itſelf ſufficient to purify tainted places and ſubſtances — why fomites are more virulent than the effluvia freſh from the ſick, &c.

* Alderſon on Contagion, p. 19, 20, 21 — Darwin's Zoonomia, ſect. xxxiii, 2, 8.

CHAP. II.

PROXIMATE CAUSE.

SECT. I.

Paſſage of Contagion.

THE manner in which the infectious matter of fever is introduced into the ſyſtem, is a point ſtill unſettled amongſt phyſicians. Authors have mentioned three different ways: 1ſt, By the pores of the ſkin. 2d, By reſpiration. 3d, By the ſaliva ſwallowed and taken into the ſtomach.

As to the firſt, by the pores of the ſkin, obſervations do not furniſh any ſatisfactory evidence of its taking place. The few inſtances which might be alleged in ſupport of this hypotheſis (arguing from the contact of a morbid with a ſound body) muſt be referred to inoculation, and not to a general abſorption of malignant vapours by the ſkin.

If the ſecond way, by the lungs, were the inlet to the febrile contagion, it is to be ſuppoſed that this viſcus would be primarily affected with the diſeaſe, which it certainly is not. The matter of

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infection, it is true, may enter the trachea and its branches with the air reſpired; but it appears, that it is immediately returned by reſpiration, diluted with the moiſture from the internal ſurface of the bronchial veſicles, and *ſuperſatured* with phlogiſton; combinations which probably alter it ſo as to obviate its noxious operation.* Beſides, from the effects of the fumes of burning charcoal, of broning brimſtone, and of other mephitick ſubſtances, it is clear, that the internal membrane of the branchiæ and veſicles of the lungs cannot bear the application of any acrid ſubſtance, without giving evident ſigns of the pulmonary functions being more or leſs diſturbed. In this manner, nature appears to have provided againſt the introduction of contagion by the lungs, otherwiſe it would have been almoſt impoſſible for any perſon to breathe in a tainted air without being infected; the contrary of which is experienced every day in hoſpitals and other apartments containing thoſe ill with contagious diſeaſes.

The third way in which it is believed that contagion gets acceſs to the ſyſtem, viz. through the medium of the ſtomach, ſeems to be the moſt probable. In the act of reſpiration the noxious efflu-

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* This ſuppoſal does not interfere with the theory of coalition between phlogiſticated air and contagion offered in the preceding ſection; becauſe a certain portion of that kind of air may be a proper nidus or vehicle for the contagious principle, whilſt a hyper-phlogiſtication may deſtroy its powers.

via are inhaled, are mixed with the ſaliva, and are conveyed by deglutition into the ſtomach, on the nerves of which they produce a certain morbid change. That oppreſſion which, on the acceſs of fever, is generally felt about the præcordia; the great ſickneſs; often a vomiting of phlegm and bilious matter, with an alleviation of ſymptoms; ſometimes a diarrhœa; ſevere head-ache; pain in the loins; and a general debility of body and mind —all conſpire to denote a primary affection of the ſtomach.* This opinion has been embraced by ſeveral phyſicians of the firſt character; one of whom only we ſhall quote, the ingenious Dr. Lind, whoſe concurrence is of the greateſt weight:— "Swallowing the ſpittle," ſays this experienced practitioner, "in infected places is juſtly deemed a means of ſooner acquiring the taint, upon which account neither the nurſes, nor any one elſe, ſhould be ſuffered to eat in the hoſpital."

* Gardner on the Animal Œconomy, ſect. iv. p. 190 & ſeq. This doctrine ſeems to be illuſtrated by the analogy of the firſt impreſſion of the variolous contagion, when taken in the natural way.—See Darwin's Zoonomia, ſect. xxxiii, 2, 10.

SECT. II.

Operation of Contagion.

THE contagion introduced into the ſyſtem, in what manner does it act? Paſſing over the ſentiments of various writers on this queſtion, I ſhall at preſent attend only to that of Dr. Ruſh, who aſſerts, that "the blood-veſſels (and not the ſtomach and bowels, according to Dr. Warren) are the ſeat and throne of this as well as of all other fevers;" and that this affection of the blood-veſſels, as he has publickly taught for ſeveral years, conſiſts in a convulſion of the arterial ſyſtem. Agreeably to the uſual idea of convulſive motion, it is difficult to comprehend how a convulſion can take place in this ſyſtem. Convulſion is the motion of a part, conſiſting of alternate contraction and relaxation, frequently and preternaturally repeated. A motion in any degree analogous to this I do not know occurring in the human body, except we admit ſpaſmodick affections of the alimentary canal to be ſuch; but theſe are contractions which do not alternate with relaxation, nor extend over the whole of that tube. Dr. Ruſh's convulſion of the arterial ſyſtem, therefore, appears to be no more than the old hypotheſis, dreſſed in new words, which ſuppoſed fever to conſiſt in an increaſed

creaſed circulation of the blood; eſpecially as the Doctor ſays, that the convulſed ſtate was moſt perceptible when the epidemick came on with a full, tenſe, and quick pulſe; for neither the intermitting, nor *ſulky* pulſe, ſeems to be his principal ſupport for the theory of arterial convulſion.

In accounting for this remarkable ſlowneſs of pulſe, Dr. Ruſh drops the idea of oppreſſed brain, and aſcribes it to "a ſpaſmodick affection, accompanied with preternatural dilatation or contraction of the heart." Yet before we grant that theſe ſuppoſed irregular ſtates of the heart are the direct cauſes of that modification of the pulſe, ſhould it not be proved, that theſe ſtates originate in the heart itſelf; that they are not the effects of ſympathy with other parts of the body; and, that their natural tendency is to induce ſuch ſpecies of movement in the arteries? But where are the proofs of theſe circumſtances to be found? Not in Dr. Ruſh's book—Are there not, however, grounds for believing, that the *ſulky* pulſe owes its origin to a morbid condition of the ſtomach, from whence the heart is ſympathetically affected, and becomes a medium through which this peculiar motion in the arterial ſyſtem is brought about?

The influence of the ſtomach on the heart and arteries is well known; and the power of *bile*, a predominant matter in yellow fever, in putting this

influence into action cannot be denied. In gout, the pulſe has been found to beat only 25 times in a minute. Many people are occaſionally affected with an intermiſſion of their pulſe for a few days, which then ceaſes again. In this caſe there is a ſtop of the motion of the heart, and at the ſame time a tendency to eructation from the ſtomach. In jaundice, a diſeaſe wherein the biliary ſyſtem is primarily engaged, the pulſe is ſometimes obſerved to be remarkably ſlow, ſo ſlow as 37 ſtrokes in a minute. And, in what diſeaſe is there better reaſon to admit the operation of the bile on the ſtomach, and its effectuating the aſſociation of motion between this viſcus and the heart, than in the diſeaſe under conſideration? *

The manner in which the infectious matter induces the ſlow pulſation of the arteries is, according to Dr. Ruſh, by cauſing *indirect* debility. Upon the principles of the ſyſtem, whoſe pathology, at leaſt, Dr Ruſh chooſes to adopt, *indirect* debility may poſſibly ariſe from a colluvies of fluids, as is ex-

* Rogers on Epidem. Diſ. p. 308 — Darwin's Zoonomia, ſect xxv. 17.—Andree on Bilious Diſ. p 39. — A remarkable ſlowneſs of pulſe has been conſidered a characteriſtick ſymptom in ſome cases of typhus gravior; as obſerved by Dr. Rogers, who ſays, that the pulſe is not in general "much moved from its natural ſtandard, either in reſpect to frequency or ſtrength; in ſome *below* it in both reſpects"—an obſervation, by the bye, which ſerves to illuſtrate the idea of the eſſential ſameneſs of all varieties of fever And here it may be aſked, ſince yellow fever often appeared at Philadelphia in the ſhape of typhus, does the former really differ in its nature from the latter?

exemplified in that overflowing of bile, which diſtinguiſhes *yellow fever;* and if this be done, it is not effected by the mixture of the contagion with the blood, but by means of the local operation of the colluvies acting on the excitability of the inteſtines, and diffuſing the aſthenick diatheſis over the ſyſtem.

Agreeably to the principles of the ſame ſyſtem (that of John Brown) not *indirect*, but *direct* debility is the cauſe of the moſt ſevere fevers, ſuch as the remittent in the warmer regions of the earth. Direct debility being the higheſt kind, and every variety of typhus, whether denominated camp, jail, or hoſpital fever, being an aſthenick diſeaſe, muſt immediately depend on the ſame ſpecies of debility, the *direct*, and therefore muſt be eſſentially the ſame diſeaſe as remittent fever, or the bilious yellow fever of Philadelphia.*

If contagion, it is true, produce direct debility, it muſt be ſuppoſed to act by a ſedative power. But the magiſterial tenet of the Brunonian hypotheſis maintains, that there is not in nature a direct ſe-

* Brown's Elements, vol. i. p. 121. Id. vol. ii. p. 294, 302. —This eccentrick, unfortunate, but ſhrewd ſyſtematick, reared a ſplendid ſtructure on the foundations of Hoffman and Cullen. In this mighty caſtle, as it were, he placed a ſeductive Magician, bearing in his hand an ebon chalice, pregnant with a matter, which, dealt out by inconſiderate youth, does more execution in one autumn, than the noted Themiſon did in ſeven; but, diſtributed by experienced age, becomes a ſource of ſalubrity and life.

ſedative. Yet contagion cannot be any other than a ſedative power, becauſe it certainly occaſions weakneſs. It is not, however, a direct ſedative; as there is no ſuch quality in nature; and to term it a ſedative in the Brunonian ſenſe of the word, would be equivalent to ſaying, that there were privations of ſome of the wonted ſtimuli of life. Contagion cannot be an exceſſive ſtimulus waſting excitability, and producing indirect weakneſs; becauſe it is aſſiſted by ſedatives, as hunger, cold, and grief, which are counteracted by ſtimulant powers. It has leſs influence over the robuſt conſtitution, than over the languid and weak; it generally operates ſlowly, and the patient feels indiſpoſition for ſome days before he is ſeized with proper fever.

It is hard to inſtance a ſingle caſe, where an exciting power induces indirect weakneſs, without intermediate excitement. Wine, like opium, firſt occaſions a certain degree of excitement; but is followed by a proportional ſickneſs, languor, and debility. "Indirect weakneſs is a waſte of excitability by exceſſive ſtimulant powers; if the excitability be ſlowly ſpent, there ſhould be a long continued excitement; if quickly, there ſhould be a ſudden and violent excitement. There can be no ſtimulus ſo powerful as to take away the whole excitability at once, as if by a ſort of exploſion, without a ſenſible intermediate excitement, unleſs like light-

lightning it cauſe inſtantaneous death. In all caſes, intermediate excitement is perceived, and if not perceived in the ſlow approach of fever, fever cannot be a ſtate of *indirect* weakneſs, nor its cauſe exceſſive ſtimuli. If contagion be neither an exceſſive ſtimulant, nor a privation of any common exciting power, no one can deny it to be a *ſedative*, otherwiſe than by the raſh *ipſe dixit* of Dr. Brown." *

The indirect debility, which Dr. Ruſh mentions, he ſeems to conceive to be the conſequence of a ferment, or chemical change in the blood. For amongſt other ſources of infection about himſelf, he includes the quantity of blood, which was ſuffered to flow and putrefy on the ground for want of a ſufficient number of bowls to receive what was drawn from the numerous patients, who were daily bled by his pupils in his ſhop and yard. † But that contagion acts in this manner in the blood, wants the ſupport of analogy, as there is no known proceſs in animal bodies, which is purely chemical, not even digeſtion.

It

* Analytical Review, vol. iv. p. 455.—I do not perceive, that it is the cuſtom to citeReviews as authorities; but, in my mind, better authorities cannot be adduced They muſt be conducted by men of univerſal information; by men of acute talents; by men converſant in every branch of ſcience, in arts, in manufactures, and in commerce. Who then can be more competent judges, who can be ſafer guides, and who can be more ſubſtantial authority? — To Reviews I do not ſcruple to apply; and to Reviews I am free to acknowledge myſelf frequently indebted and obliged.

† Ruſh on the Bilious Yellow Fever, p. 353.

It appears that contagion is propagated not by means of the circulation, but by ſympathy of diſtant parts with each other; "ſince if a diſtant part, as the palate, ſhould be excited by ſenſitive aſſociation * into the ſame kind of motions, as the parts originally affected by the contact of infectious matter; that diſtant part will produce the ſame kind of infectious matter; for every ſecretion from the blood is formed from it by the peculiar motions of the fine extremities of the gland, which ſecretes it; the various ſecreted fluids, as the bile, ſaliva, gaſtrick juice, not previouſly exiſting, as ſuch, in the blood-veſſels."

As this reaſoning applies to contagious diſorders, reiterable as well as unreiterable, it is equally applicable to fever as to ſmall-pox. With regard to variolous contagion, it does not appear to affect the conſtitution by a fermentation or chemical change, but by producing new motions of the cutaneous terminations of the blood-veſſels, which, for a time, proceed daily with increaſing activity, like ſome paroxyſms of fever, till they at length ſecrete, or form a ſimilar poiſon by theſe unnatural actions. If then variolous contagion act thus on the moving powers only, is it not at leaſt equally reaſonable to conclude, that febrile infection, which probably is a more ſubtle and diffuſible matter, will

ope-

* Senſitive aſſociation conſiſts in motions, whether muſcular or ſenſual, which become connected by habit.

operate entirely on the ſame powers? The blood of perſons in the ſmall-pox will not communicate the diſeaſe. And as a convincing proof that the fluids are not primarily affected in fever, ſeveral nurſes labouring under malignant fever have continued to give ſuck from beginning to end of the diſeaſe without imparting it to the children.*

If all the fluids were tranſmuted into a contagious maſs, no patient could recover from a fever of a fortnight's continuance. Nor would a patient, after recovering from the diſeaſe, ceaſe to infect others, until the whole maſs of his fluids were changed. Neither would peſtilential diſtempers be ſo eaſily cured, as we ſometimes ſee them, by adminiſtering a ſmall quantity of bark and wine; which can only be ſuppoſed to act, by ſubduing the morbid ſtate of the moving fibres. †

Even the vehicle or nidus of contagion, which we have conceived to be a certain ſpecies of phlogiſticated air, appears to operate in its diſengaged ſtate on the moving powers of the body; and may, therefore, inſtead of being an opponent, ſerve as an aſſiſtant to contagion. The phœnomena, attending the extinction of life in thoſe to whom ſuch gaſes have proved mortal, ſhew that the poiſon

* Darwin's Zoonomia, ſect. xxxiii. 9, 10.—Dickinſon's Inquiry into the Nature and Cauſes of Fever, chap. viii.

† Ferriar's Med. Hiſt. & Reflect. p. 237, 38.

operates chiefly on the nervous ſyſtem. "The vital principle," ſays the worthy and ingenious Dr. Percival, "ſeems to be arreſted and almoſt inſtantly deſtroyed; ſometimes even without a ſtruggle, and poſſibly without an antecedent pain;" and this opinion is corroborated by obſerving, that the means found moſt ſucceſsful in procuring reſuſcitation are thoſe which operate upon that principle.*

Another hypotheſis of the acting quality of contagion, produced by a very ingenious writer, ſuppoſes that the poiſon of typhus, and conſequently that of every fever, operates by depriving the ſyſtem of oxygene. This opinion is founded on the belief of the exiſtence of a ſimilarity between the nature of typhus and that of ſcurvy. But were there no better grounds for the doctrine than this aſſumed ſimilarity, it would not continue valid; becauſe obſervation is ſtrongly againſt it. For, inſtead

* The relation between contagion and thoſe phlogiſticated vapours may be ſtill more readily admitted, if, as Mr. St. John imagines, and we ſuſpect, the poiſon of fevers be itſelf a *peculiar gas* exhaled from the ſurface of the ſick. In this caſe, ſome fortunate diſcovery may poſſibly furniſh us with a method of neutralizing it, ſo as to prevent it from infecting a patient's aſſiſtants. But it has been ſuppoſed, that experiments of this kind are too difficult and hazardous. It may, however, be aſked, are not theſe objections ſomewhat leſſened by conſidering, that the known means for mitigating contagious virus, as ablution, cleanlineſs, and ventilation, may be ſo combined in the experiments, as to render the danger no more than in common attendance? (See Ferriar's Med. Hiſt. & Reflect. p. 240) —It may not be incurious here to obſerve the correſpondence between an effect of thoſe gaſes, and one of contagion, as noticed by Brown, Beddoes, and others, namely, that, in ſome inſtances, BOTH produce a ſtate of intoxication.

ſtead of any degree of analogy ſubſiſting between the two diſtempers, it is a certain fact, that the ſtate of the ſyſtem, which takes place in ſcurvy, abſolutely reſiſts the acceſs of fever.*

If by depriving the ſyſtem of oxygene, Dr. Beddoes conceives, that this effect is brought about by the contagion undergoing a chemical change in the blood, the above reaſoning renders the exiſtence of ſuch a proceſs highly improbable. But if Dr. Beddoes means, that contagion diminiſhes oxygene, or the baſis of vital air, by its operation on the irritability of the ſyſtem, I join iſſue with him in this conſtruction.

In a paper read by Dr. Wood before a Society in Newcaſtle, and publiſhed in 1793, a theory is broached which appears to be a mixture of that inculcated by Dr. Brown with that ſuggeſted by Dr. Beddoes. Dr. Wood thinks the proximate cauſe of fever conſiſts in "an over-proportion or accumulation of carbone and hydrogene, and an exhauſted ſtate of irritability." On this hypotheſis, particular remarks would be ſuperfluous; ſince the ſources from whence it is principally derived have been already conſidered.

* Lind on Fevers and Infection, p. 11.

CHAP. III.

METHOD of CURE.

SECT. I.

Of Purging.

AT the beginning of the fever, Dr. Buſh informs us, that moderate bleedings and ſmall doſes of mercurial purges were in ſeveral caſes ſucceſsful; but that they afterwards failed; and that he was baffled in every attempt to ſtop the ravages of the diſorder, until he was encouraged to adopt the theory and practice which ſanctioned the violent evacuating plan.*

On the authority of Clark, Pringle, Cleghorn, and Balfour, he had conſiderable dependence; but his chief voucher for the ſafety and efficacy of calomel and jalap, in doſes he calls *ten* and *ten*, was the practice of Dr. Young, a ſenior ſurgeon in the hoſpitals in the late war. Much earlier authority, however, might have been found, or recollected, for the exhibition of mercury, both on partial and general indication. Sir Theod. Mayerne, a hun-

* Ruſh on Bilious Yellow Fever, p. 192 & ſeq.

hundred and fifty years ago, beſtowed particular encomiums on calomel, given to the extent of twenty grains as a purge. And more than fifty years have paſſed ſince it was highly commended, by Dr. Gilchriſt, (to whoſe works Dr. Ruſh is no ſtranger) in inflammatory complaints and fevers. * This practice was afterwards transferred to the cure of the liver-diſeaſe, in the Eaſt and Weſt Indies; and from an apparent analogy, being applicable to bilious fever, it has ſince been a remedy in the hands of almoſt every practitioner—conſequently, as a remedy in theſe kind of diſeaſes, it is not entitled to the diſtinguiſhed name of a *new medicine*. Nor does it appear, that, even on the occaſion before us, the honour of the firſt eſſay with mercury is due to Dr. Ruſh; for by many it is aſcribed to Dr. Hodge and Dr. Carſon, who are ſaid to have employed it a week before him. †

To try, however, the merits of the evacuating ſcheme by every teſt in our power, it ſeems neceſſary to begin with a ſurvey of the principles of the Ancients reſpecting the uſe of purging in fevers. In this reſearch, Lommius, who wrote a treatiſe on the cure of continued fevers, in 1562, appears to be a proper guide; eſpecially as he is much commended, and in particular for the correctneſs with which

* Letters on the Internal Dropſy of the Brain, by the Author of theſe Remarks, p. 44.

† Carey's Account of the Yellow Fever, chap. iii.

which he has ſtated the ſeveral rules for managing evacuations. If it be objected, that the Ancients, and their commentator, Lommius, are not fit ſtandards by which a modern line of practice ought to be meaſured, becauſe they believed in the doctrine of concoction and excretion; it may be anſwered, that for this very reaſon they are the beſt authorities, ſince their habit of watching for the critical moments, *(ſtatus egritudinis)* gave them excellent opportunities of becoming good judges in the adminiſtration and effects of medicines.

Our author condemns thoſe, who, in fevers ſcarcely let a day paſs without giving purgatives by the mouth or by the anus; and enumerates a variety of bad ſymptoms, which are likely to be the conſequence. He thinks it, therefore, an inexcuſable practice, in a fever which would end on the 9th day, to give a purge on the ſecond, and then to take away a pound of blood or more; on the 3d, to inject a clyſter; on the 5th or 6th, to bleed again; and on the 8th, to drench with another purging potion. In ſuch a routine it is no wonder, that the diſeaſe, ruling over the exhauſted powers of nature, occaſions delirium, ſopor, &c. and terminates in death before the 14th day. *

In

* "Scribit Hippocrates purgantium medicamentorum potiones, in morbis acutis, raras eſſe oportere." — "Per febres, inquit Celſus, medicamentorum dari potiones et alvus duci, non niſi raro, debent; quoniam ex imbecillitate ſummum periculum eſt." Lommius de Febribus, p. 122, 124.

In the advanced ſtate of fever, when delirium, reſtleſsneſs, &c. come on, he adviſes a ſmall doſe of a purging medicine, juſt as much as may ſerve to operate on the biliary paſſages, by which nature would be enabled to expel the more remote reſiduum; but, if there be reaſon to apprehend the riſe of a fatal inflammation in an important part, he would purge immediately, without waiting for concoction; at the ſame time inſiſting, that the purgatives ſhould be of the mildeſt kind, and that ſometimes lenient injections are equally uſeful—The periods for adminiſtering theſe remedies, he tells us, are in the intermiſſions, becauſe in the exacerbations, they produce inquietude, break the ſtrength, and occaſion determination to the head. *

From this ſummary view of the ancient, we advert to the modern practice, and find, that, in Jamaica, Dr. Hunter commenced the cure with a purgative medicine that would operate mildly but effectually, for which purpoſe he preferred an ounce of Natron Vitriolatum. After a few ſtools were thus procured, the patient generally found himſelf eaſier, and a remiſſion enſued. When this took place, recourſe was immediately had to the Peruvian

* "Scito minus noxæ fore, cum reſiduum materiæ, quam neceſſe vacuatur, eoque pervenitur, ut vires periclitentur. Nam ſæpiſſime natura hoc reſolvit reſiduum. Ea ſint inidonea medicamenta, quæ materiam morbi ex toto corpore radicitus extrahunt. Datur antem modo per os, modo alvo injicitur." Lom. de Feb. p. 129, 136. — It is curious to obſerve, that the ſudden ſubſtraction of morbid ſtimuli is here condemned.

vian Bark; and, if it happened to diſagree, wine, coffee, milk, or the like, as beſt ſuited the ſtomach, were uſed in its ſtead. When it was more difficult to accompliſh a remiſſion, he uſed ſmall doſes of James's powder; followed alſo by the Bark. To palliate ſymptoms, he employed chamomile tea, effervеſcing draughts, ſmall doſes of laudanum, and moderate quantities of wine—But not a word of violent purges—not a word of enormous veneſection!

In the putrid inteſtinal fever of Bengal, without local affection, whether mild or ſevere, Dr. Balfour, one of Dr. Ruſh's authorities, begins with ſmall doſes of calomel aſſiſted by a gentle ſaline purgative; and in two days adminiſters the Peruvian Bark; alternating the former with the latter, till the fever give way—With a view to prevent the Bark from being thrown up, or paſſing down, he preſcribes opiates freely.

In the ſame manner Dr. Balfour proceeds, when the fever is mild, though attended with local affection; but when this affection is obſtinate, eſpecially if ſeated in any important part, he employs bleeding and bliſtering; taking care, nevertheleſs, to obviate the failure of the general ſtrength, which is apt to ſucceed the loſs of blood, by an immediate exhibition of Peruvian Bark, wine, and nouriſhment.

Dr.

Dr. Clark aſſerts, and his opinion is reſpected by Dr. Ruſh, that nothing is more indiſpenſibly requiſite, in the beginning of bilious fever, than to cleanſe the inteſtinal tube by *gentle* vomits and purges. Nature, he thinks, always indicates the neceſſity of ſuch evacuations by the copious ſecretion of bile, which, if not ſpeedily diſcharged, often brings on an inflammation of the ſtomach, nauſea, and hiccough; preventing, in the courſe of the diſeaſe, the effects of the moſt powerful medicines. In concurrence, then, with nature, in mild caſes, he uſes for the purpoſe of evacuation Ipecacuanha, with a little tartariſed Antimony; and if theſe do not move the belly, he next day preſcribes a doſe of neutral purging ſalts; but in ſevere caſes, he confides in frequent diminiſhed doſes of the Antimonial, rendered more certainly laxative by the addition of manna, decoctum tamarindorum, or a ſmall portion of ſal catharticus.

In theſe evacuations he did not perſiſt long, as he conſidered it vain to expect that they would prevent a generation of bile; for ſo long as the feveriſh indiſpoſition continues, although an emetick and a cathartick were repeated every day, more ſordes will be generated. No ſooner, therefore, was the inteſtinal tube thoroughly cleanſed, than he had recourſe to Peruvian Bark, which he preſcribed in as large doſes as the patient's ſtomach would bear, and on which, he inſiſts, that the principal part of

the cure depends. But if, after evacuations, the ſtomach remain weak and ſqueamiſh, which is frequently the caſe in bad fevers, he often found the greateſt advantage from adminiſtering a full doſe of ſolid Opium. And, if the patient's ſtrength began to ſink, he ſupported him by giving wine liberally in his drink, food, and medicine.

More authorities for lenient purging might be adduced from modern writers; but on thoſe few I ſhall reſt the merits of the practical point here maintained, corroborated as they are by the reſpectable opinions recited in Dr. Ruſh's book. Here we find the experienced phyſician before mentioned, from St. Croix, lay it down as a precept, that all violent catharticks ſhould be avoided; and that they are injurious in the advanced ſtage of the diſeaſe. If the bowels are not ſufficiently open, he adviſes a laxative clyſter, or a few grains of Rhubarb to be added to each doſe of Bark, until the deſired effect be produced. Dr. Kuhn does not give a laxative, unleſs indicated by coſtiveneſs, when he recommends cream of tartar or caſtor oil; but he prefers a clyſter to either of theſe medicines. The manuſcript which Dr. Ruſh got from Dr. Franklin inculcates a ſimilar practice, and declares that the evacuation from the bowels "muſt be procured by *lenitive chologogue* purges." Beſides, Dr. Ruſh himſelf, in his ſcheme of directions depoſited with the Apothecaries, after four or five large mo-

motions are obtained by his three cathartick powders, adviſes the bowels to be kept ſoluble either by one powder, or by ſmall doſes of cremor tartaris, or cooling ſalts, or by common opening injections.

From theſe conſiderations, I am led to conclude, that neither the draſtick purge, which provoked 23 ſtools and occaſioned fainting in advanced age, nor a plentiful ſpontaneous diarrhœa, at any ſtage of the diſorder, can be the ſafe and ſalutary evacuations which they are pronounced to be by Dr. Ruſh. On the contrary, it is manifeſt, that the precepts and practice, both of ancient and modern times, are decidedly againſt the Doctor's ſcheme of purging. Hence I muſt ſubſcribe to the objections made againſt it, as recorded in his book, namely, that his preparation is of too draſtick a nature, is comparable to arſenick, and is a doſe for a horſe; that, as exciting ſalivation, it is hurtful; that, as adding to the excoriation of the rectum, it contributes to increaſe inflammation of this part; that it tends to lacerate and inflame the bowels; and that being preſcribed indiſcriminately, it muſt do harm in weak habits.

SECT. II.

Of Blood-Letting.

Dr. Ruſh preferred the uſe of veneſection in the exacerbation of the fever; but he let blood in ſeveral inſtances in the remiſſion, where the pulſe was tenſe or corded; and he had recourſe to it always when the pulſe was ſlow—when the fever appeared in a tertian form—and when the patients were able to walk about, provided the pulſe was full and hard. He paid no regard to the diſſolved ſtate of the blood, even when it appeared on the firſt or ſecond day of the diſorder, but repeated the bleedings afterwards in every caſe, where the pulſe continued to indicate it. Nor did the preſence of petechiæ deter him from repeating the evacuation, where the pulſe retained its fulneſs or tenſion.

In determining the quantity of blood to be drawn, he was not only governed by the ſtate of the pulſe, but by the temperature of the weather. In the beginning of September, he found one or two moderate bleedings ſufficient; but afterwards the evacuation required to be more frequently performed. Many patients be bled twice, and a few three times a day. At one period he preferred frequent and ſmall, to large bleedings; whilſt at

other

other periods, he ſaw no inconvenience from the loſs of a pint, and even of twenty ounces at once. From many perſons he drew ſeventy and eighty ounces in five days; from a few, an hundred and an hundred and fourteen ounces—Children, and even old people, bore the loſs of blood remarkably well; and he was not deterred from its uſe by debility, whether habitual or the effect of previous diſeaſe. *

Conformably to the plan followed in the preceding Section, when treating of Catharticks, I ſhall here give a ſketch of the ancient doctrine reſpecting Veneſection. Our earlier predeceſſors generally carried this evacuation to an enormous length, even to fainting; but never puſhed it ſo far without conſidering the age, the conſtitution, and the ſtrength of the patient. They recommended its uſe in the beginning of fevers, in adults and youth, in whom vigour and blood naturally abound; but they reckoned it dangerous to repeat it in the increaſe or achme; and they conſidered it always improper in diarrhœa and hœmorrhage.

When the patient's ſtrength is not much exhauſted, but is merely a little diminiſhed with lowneſs of ſpirits, veneſection, they tell us, is neither to be entirely omitted, nor the quantity of blood drawn to be copious; adviſing rather to make the eva-

* Ruſh on Yellow Fever, p. 267, 272.

evacuation ſmall, and to repeat it occaſionally; whilſt in the intervals the ſtrength of the patient is to be ſupported by proper nouriſhment. The length of the intervals (which may ſometimes be ſhort) is to be regulated by the ſtrength of the patient, and the ſtate of the diſeaſe. In great weakneſs, however, the lancet is to be entirely withheld, eſtimating this condition principally from the ſituation of the vital powers; whilſt conſiderable caution is uſed in judging from the pulſe, as it is liable to be affected by a variety of circumſtances, which are often quite unconnected with the diſeaſe.

The quantity to be drawn depends upon the reſult of a compariſon between the diſeaſe, and the ſtrength of the patient. In a violent diſeaſe, if the patient's ſtrength be not impaired, a liberal evacuation is adviſed, eſpecially in ſynochus; and the bleeding to ſyncope, according to Hippocrates and Galen, is juſtified on the principle, that the ſudden emptying brings the patient directly to a ſtate different from what he had been in juſt before, whilſt he may be revived from the fainting by refrigerating means. Others again do not accede to this doctrine, but prefer taking away moderate portions of blood at certain intervals. Beſides, to ſome patients, who do not readily faint, it is obſerved, that an enormous evacuation might be death.—Upon the whole, it is to be regulated by various conſiderations, viz. by the ſtrength, temperament,

habit

habit, and age of the patient—by the ſeaſon of the year, and by the climate of the place. *

The circumſpection, which in general is inculcated in this part of the cure, militates againſt the ſtrong and precipitate practice, to which Dr. Ruſh ſeems to be too much devoted; and againſt which modern experience lifts up her voice. Dr. Blane thinks, that more harm has been done, in treating yellow fever, by being too officious, than by a diffidence in art, and truſting to the powers of unaſſiſted nature. This diſeaſe, he ſays, does not bear evacuations, excepting in the very beginning; nor is there the ſame neceſſity for them, as there is in the common remitting fever. One of the great difficulties in the cure is, a conſtant and unconquerable irritability of the ſtomach. In abating this ſymptom, he found a bliſter applied to the region of the ſtomach more ſerviceable than any other remedy. And when the ſtomach could be brought to bear medicine, a free uſe of the Peruvian Bark was evidently beneficial, and was indeed the only preſcription to be relied on.

Dr. Clark declares, that he is convinced both from his own experience, and from the information he received from others, that the remitting fever is every

* Lommius de Febrib. p. 4, 64.—Here again it is worthy of notice, that enormous blood-letting is above juſtified on ſomewhat the ſame principle, as that upon which Dr. Ruſh proceeds, namely, the quick abſtraction of morbid ſtimuli.

every where the ſame, and requires the ſame treatment. Although he thinks, that the genus of this fever does not require bleeding; yet, as this evacuation had been ſo generally recommended by eminent phyſicians, particularly by Drs. Huck and Cleghorn, he was induced to open a vein during the firſt paroxyſm in three patients. The conſequence was, the firſt did not bear the evacuation, his pulſe flagged, and he was very delirious in the enſuing fit. The other two, who were ſeized ſuddenly with delirium, returned to their ſenſes on performing the operation; but before five or ſix ounces of blood were taken, they became faint, and the feveriſh paroxyſm ran higher than in thoſe who did not ſuffer the evacuation. For the future he reſolved to be very cautious in blood-letting; and, ſince that period, he laid it aſide in every fever in warm climates, both at ſea and on ſhore, unleſs accompanied with topical inflammation.

In cold and temperate climates, indeed, he grants that veneſection may ſometimes have a good effect; at leaſt taking away ſome blood in the beginning of a fever, he thinks cannot be attended with great danger. It may likewiſe be uſed at the commencement of warm weather, when many of the fevers are ſo mild as to require almoſt no other cure, than to cleanſe the primæ viæ, and to produce an equable perſpiration by relaxants. But when a perſon has continued a ſhort time in a warm cli-

climate, he pronounces that blood-letting is extremely detrimental. "I have frequently been induced to try it," ſays he, "when it ſeemed to be ſtrongly indicated by great drought, head-ache, flushed countenance, and oppreſſed pulſe; but I ſeldom ever ſaw it anſwer any good purpoſe."

Dr. John Hunter found, that blood-letting did no good even in thoſe caſes which ſeemed to require it moſt, and, if copious or repeated, was always hurtful; and that too where it ſeemed neceſſary to bleed freely on account of pulmonick inflammation. Dr. Balfour alſo, whoſe principal dependence, as before obſerved, is placed on other means, adviſes bleeding, but to be joined with bliſtering, provided they be indicated by the violence and obſtinacy of the local affection, eſpecially when ſeated in any vital or important part; attending at the ſame time to the precautions, mentioned already, for preventing the bad effects which are apt to follow the evacuation.

Theſe few authorities being forcible and ſatisfactory, I need not multiply evidences here, any more than in the preceding Section, to counteract the rapidity with which Dr. Ruſh ſeems in general too much diſpoſed to waſte the vital current. It is true, he ſpeaks of the gradual abſtraction of ſtimuli, and of one or two moderate bleedings being ſufficient at a certain ſeaſon to ſubdue the fever; but his propenſity to a profuſe extraction of blood

is every where apparent, eſpecially where, in addition to many other inſtances, he tells us of the immenſurable quantities which he ordered to be drawn from the poor, who were able to walk to his houſe. At the ſame time, he owns that he had examples of veneſection being not only hurtful, but fatal. In the beginning of September, two of the patients, who had been bled without his knowledge, appeared to have fallen victims to the operation; and he heard of a man, who had been bled on the firſt day of the diſorder, and who died in 12 hours afterwards. Theſe deſtructive caſes produced temporary caution in Dr. Ruſh, but they did not deter him from purſuing his ſyſtem of profuſe bleeding, as ſoon as the diſeaſe was ſuppoſed to have changed its type.

In this point, then, as in the former, until I have good reaſons for the contrary, I muſt concur with the objectors who contended, that bleeding was unneceſſarily copious, and proved often deſtructive; that many of the indiſpoſitions, and much of the ſubſequent weakneſs of perſons, who recovered after the free uſe of the lancet, might be aſcribed to it; and that blood-letting was preſcribed indiſcriminately, without paying ſufficient regard to age, conſtitution, or the force of the diſeaſe. Beſide, with the boaſted efficacy of both the favourite remedies, purging and bleeding, how ſhall we reconcile the following confeſſion? "Under every mode of treatment, it (the fever) ſeemed diſpoſed after it was completely formed to run its courſe." *

* Ruſh on the Bilious Yellow Fever, p. 266.—Dr. Ruſh's prac-

SECT. III.

Of cool Air, cold Drink, Bliſters, and other Remedies.

IN purging and bleeding, however highly prized, Dr. Ruſh did not place excluſive confidence. The theory of the proximate cauſe, which he had adopted, led him to uſe cool air, cold drinks, low diet, and applications of cold water to the body.

After employing evacuations, the ancients were fond of attending to the uſe of refrigerants in fevers. Every method to cool the patient, eſpecially by means of cold air, was ſedulouſly purſued. Even in temperate weather, the air in the ſick chamber was fanned by opening the door and windows; and if this were not enough, the floor was ſprinkled with vinegar and water, ſtrewing it over likewiſe with freſh and fragrant branches. A crowd of people, as contributing to heat the room, was prohibited; and it was further cooled by pouring water frequently from one veſſel to another. Theſe expedients, on the principle of refrigerating the ſyſtem through the medium of the lungs, were

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practice was diametrically oppoſite to that of the French phyſicians; and yet he confeſſes, p. 243, that they had a great influx of patients, owing not only to the diſſentions among the American practitioners, but to *a general opinion of their ſuperior knowledge in treating the diſtemper*—a fact which gives no ſmall additional weight to our animadverſions.

preferred to the application of cooling things to the ſurface; becauſe it was ſuppoſed, that by reſpiration the heat of the heart was abated, whilſt the external remedies might prevent the tranſpiration of the morbifick matter through the cutaneous pores, or determine it to the important viſcera. *

For the ſame reaſon, it is probable, that the uſe of the bath was forbidden in the firſt days of the diſorder; and that cold applications to the hotteſt parts only were recommended; but, if the patients were chilly, theſe likewiſe were omitted—making, however, as Dr. Ruſh did, low diet an article in the treatment. † The Doctor found the ancient practice of introducing cool and freſh air equally proper, whether the arterial ſyſtem was depreſſed or excited; it ſometimes had the effect of diminiſhing the pulſe without bleeding; and it ſometimes had the reputation of being the principal means in accompliſhing a recovery. ‡

Be-

* "Nonnulli, Galenum ſecuti, aquam frigidam de vaſis in vaſa transfundunt, quæ res ad ſomnum etiam conciliandum facit." Lom. de Feb. p. 138, 141. — Theſe effects may be explained by the property of water, eſpecially when agitated, in ameliorating phlogiſticated air; and perhaps partly by the power which water is found to have in diluting certain contagions. See Prieſtley on Air, and Darwin's Zoonomia.

† "Primus diebus balneo quidem abſtineat, vino autem et oleo tepido in cubili illinito, cibis abſtineat, ſorbitiones autem tenues et frigidas exhibito. Cumque ardor tenuerit, lintea frigida intincta qua præcipue ardere dixerit admoveto. At ſi horror corpus detinuerit, refrigerantia remittito." Hippocrates de Intern. Affect. ſect. v. p. 553.

‡ Ruſh on the Yellow Fever, p. 284, 5, &c.

Beſides evacuating and cooling meaſures, Dr. Ruſh uſed other remedies. After the reduction of the ſyſtem, *bliſters* were applied with great advantage to every part of the body; laid on the epigaſtrick region, they were ſometimes efficacious in ſtopping thoſe moſt diſtreſſing ſymptoms, the vomiting and burning ſenſation of the ſtomach; and put to the crown of the head, they were particularly ſerviceable. Where the ſtomach was affected with pain, unattended with vomiting, a mixture of liquid laudanum and ſweet oil, uſed outwardly, procured relief; but againſt the pain itſelf, he found no remedy equal to a table-ſpoonful of ſweet milk taken every hour, or to ſmall draughts of milk and water; and where milk failed of giving relief, he preſcribed ſmall doſes of ſweet oil, or a mixture of milk, ſweet oil, and molaſſes.

Theſe are the only points which circumſtances permit me to conſider at preſent; others remain for a future opportunity, when I propoſe to enquire, Whether, in obedience to Sydenham, the whole practice in fevers ſhould be abſolutely limited to bleeding, purging, and ſmall beer? How far intercurrents and epidemicks mutually affect each other? How applicable mercurial ſalivation is in the cure of fever? And how ſalutary it may be to make a ſudden and total change in diet, as a preſervative againſt the deſtructive effects of contagion?

APPENDIX.

SINCE the preceding pages were written, I have had the pleaſure of reading a very intereſting publication, entitled, "An Eſſay on the malignant Peſtilential Fever introduced into the Weſt Indian Iſlands from Boullam on the coaſt of Guinea, as it appeared in 1793 and 1794. By C. Chiſholm, M. D. and Surgeon to his Majeſty's Ordnance in Grenada."

By this intelligent and experienced practitioner, we are aſſured, that the diſeaſes in Philadelphia and in the Weſt Indies "were exactly the ſame;" a convincing proof of which, he deems the following fact: "A veſſel belonging to Philadelphia introduced the diſeaſe into St. Pierre, Martinique, in October 1793. Another veſſel from New London touched at St. Pierre, in her way to Grenada, and received the infection. On her arrival, in February 1794, the ſick were put under my charge; and I found the diſeaſe to be my old enemy, the malignant peſtilential fever. I treated it with mercury, and was ſucceſsful."

Upon a comparative view of the method of cure commended by Dr. Chiſholm, with that extolled by Dr. Ruſh, they appear to differ eſſentially

ly in the moſt material points. Dr. Chiſholm finds it neceſſary to clear not only the bowels, but the ſtomach at the beginning of the fever; for which purpoſe, he unites an emetick and cathartick, a neutral ſalt and an antimonial, and adminiſters them at the ſame time. Dr. Ruſh rejects the emetick, and depends ſolely on the cathartick, generally compoſed of calomel and jalap. The former phyſician pronounces blood letting a dangerous expedient, and on no account admiſſible; the latter conſiders it the moſt ſalutary remedy, contends for its being conſtantly neceſſary, and is fond of recommending a copious diſcharge by its means.

To obviate inflammatory diatheſis, inſtead of veneſection, Dr. Chiſholm relied on a pill conſiſting of five grains of calomel, two of antimonial powder, and one of opium, which he repeated four times in the twelve hours, or eight times in the twenty-four hours; and he alſo adminiſtered opium by injection, to take off the irritability of the ſtomach. On the other hand, Dr. Ruſh exclaims againſt the internal uſe of opium in any form, or in any quantity. The former ſpeaks favourably of Anguſtura Bark and Æther; the latter decries all tonicks and ſtimulants. The one found that Peruvian Bark could be rendered uſeful by the addition of brandy, wine, opium, or æther; the other condemns it, with all its combinations, and every kind of cordial.

Mer-

Mercury given to produce a ptyaliſm became a primary object with Dr. Chiſholm; whilſt, for that purpoſe, it was no more than a ſecondary conſideration with Dr. Ruſh, having adminiſtered it principally as a purgative medicine. Bliſters, in the hands of the former, whenever applied, or with whatever intention, were of little or no uſe; whereas, in the practice of the latter, after the reduction of the ſyſtem, they were laid with great advantage on every part of the body.

Since, then, in the very ſame diſeaſe, we are preſented with remarkably contrarious rules of practice by two eminent phyſicians, I would infer as follows: That one or other ſcheme muſt be wrong; that both ſhould be examined attentively before either be admitted; and, conſequently, that the Remarks contained in the foregoing pages receive additional juſtification.

☞ As Dr. Chiſholm, for want of ſufficient ſanction, ſeemed to enter upon the uſe of Mercury with a heſitating anxiety, I would beg leave to refer him alſo to *Letters concerning the internal Dropſy of the Brain*, p. 44, 53, 54, where, in addition to the authorities which he himſelf advances, he will find farther ſupport, deduced eſpecially from his chief pillar, Dr. Gilchriſt.